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PART I.—ESSAYS, MONOGRAPHS, AND CASES.

The Physiology of the Fœtal Circulation. By E. R. PEASLEE, A. M., M. D.,
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FœTAL Anatomy has, for several years past, received a large share of attention, while fœtal physiology has hitherto been, to a great extent, neglected, except so far as mere development is concerned. It is, indeed, a department of no ordinary difficulty, since the human fœtus passes through various successive phases of function, as well as development. In regard to its circulation, for example, it is at one time analogous to a fish; subsequently to a reptile; and does not become a true mammal, indeed, till after birth.

Still, we may now enter this field with views somewhat extended; and it is the object of this paper to show that the view of the fœtal circulation, which is adopted by the highest authorities, is unsatisfactory and incorrect; and to propose such a view as the present state of science demands.

In this view the last half of fœtal life alone is included; and it is proper to commence with a statement of the ideas, both in regard to the course and to the physiology of the fœtal circulation, which are generally entertained.

I. *The course of the Fœtal Circulation.*

About 150 years ago a violent controversy arose in France, and extended to the neighboring kingdoms, respecting the manner in which the blood passes between the auricles, through the foramen ovale.* It was excited by Meri's

* For the arguments adduced in this controversy, see Senac's *Traité de la Structure du Cœur*, tome 1, p. 369; and the *Supplement*, in tome 2.

theory—that it passes from the left to the right auricle—while Harvey had assigned to the current an opposite direction. Boerhaave, Lancisi, Winslow, and others, investigated the subject; and their conclusions have undergone but slight modifications up to the present time.

The course of the foetal circulation as usually stated—independently of the inferences generally associated with the facts—is as follows:

1. The aerated, or arterial, blood returned by the umbilical vein, from the placenta through the umbilicus, and to the inferior surface of the liver, is thence poured into the inferior vena cava; a part having entered that vessel at once through the ductus venosus,* and the rest having first been transmitted through the liver.

2. The blood in the inferior vena cava is poured into the right auricle, and by the *Eustachian valve turned across into the left auricle through the foramen ovale*.

3. While the left ventricle is thus filled, the right is *filled by venous blood*, from the superior vena cava.

4. The mostly *arterial* blood is sent from the left auricle through the left ventricle into the aorta, and *principally through the branches from its arch* to the head and upper extremities; the *venous* blood in the right auricle is sent through the right ventricle into the trunk of the pulmonary artery; thence in very *slight degree* to the collapsed lungs, but *almost entirely* through the ductus arteriosus into the descending aorta—there to mingle with the *purser* blood which has passed down through the arch of the aorta. 5. This mixed blood is distributed to all the parts supplied in the adult by the descending aorta; and the blood thus carried to all parts below the diaphragm is returned to the right auricle by the inferior vena cava. 6. But the internal iliac arteries are not only distributed in the foetus as in the adult. They are also prolonged on each side of the bladder, and above it to the umbilicus, and through the latter into the cord, and to the placenta. These prolongations are called the “umbilical arteries;” and by them the blood is returned from the foetus to the placenta.

This view is illustrated by Fig. 1, here taken from Carpenter's Physiology; and which is placed on another page for the sake of contrasting it with the correct representation of the parts in Fig. 2. Some of the words in the preceding paragraph are also italicized, since they convey incorrect or false ideas, and will be particularly remarked upon in another connection.

II. *The Physiology of the Foetal Circulation, as at present understood.*

1. The blood arriving in the inferior cava from the umbilical artery, “having been thus transmitted through the *two* great depurating organs—

* This vessel extends from the umbilical vein directly into the vena cava.

the placenta and the *fetal liver*—is in the condition of arterial blood; but being mixed in the vessels (the inferior cava) with that which has been returned from the trunk and lower extremities, it loses this character in *some degree*, by the time that it arrives at the heart.”—Carpenter’s Human Physiol., 4 Ed., p. 997.

2. This mixed blood is prevented, “in *great degree*, if *not entirely*,” from farther admixture with the venous blood in the right auricle, by the *peculiar action* of the Eustachian valve; which carries it at once through the foramen ovale into the left auricle.—Ditto.

3. The ventricles contracting, the left propels its “*arterial*” blood into the ascending aorta, and supplies the head and upper extremities *before undergoing any admixture*; while the right ventricle sends its *venous* blood through the (trunk of the) pulmonary artery, and the *ductus arteriosus*, into the *descending aorta*, there to mingle with the pure blood just mentioned, and be distributed to the trunk and lower extremities.—Ditto.

4. “Thus the head and superior extremities, whose development is required to be in advance of that of the lower, are supplied with blood *nearly as pure* as that from the placenta;” and *vice versa* as to the rest of the body.—Ditto, and Wilson’s Human Anatomy, p. 555.

5. “In the adult the blood would be circulated through the lungs and oxydated; but in the *fœtus* the lungs are solid and *almost impervious*. Only a small quantity of the blood passes, therefore, into the lungs; the *greater part* rushing through the ductus arteriosus,” &c.—Wilson, *ut sup.*, p. 553.

6. “The pure blood from the placenta is distributed in considerable quantity to the liver, before entering the general circulation. *Hence*, the abundant nutrition of that organ, and its enormous size in comparison with other viscera,” in the *fœtus*.—Ditto.

We find essentially the same statements in all the best authors of the present time; and, therefore, no farther quotations or authorities are deemed necessary. We have here, also, italicized such expressions as need particular consideration, on account of their entire or partial incorrectness.

Each of the preceding six propositions will be separately reviewed, with the intention of showing their incorrectness, and at the same time of demonstrating the only true view of the subject. A few remarks will, however, be premised in this connection, expressive of the author’s doubts as to the peculiar—and really incredible—action universally assigned to the Eustachian valve.

The assumed function of the Eustachian valve not probable.

Some experiments, intended to elucidate this subject, and to which a great deal of importance seems to have been attached, were performed by

the late Dr. John Reid.* The following was regarded as the most satisfactory: Having injected both the venæ cavæ, of a foetus of seven months, at the same time—the superior cava with yellow, and the inferior with red, injection—he found that the “red had passed through the foramen ovale, and filled the left side of the heart without any intermixture with the yellow, except very slightly at the posterior part of the right auricle. Not a drop of the yellow appeared to have accompanied the red into the left side of the heart. From the left side it (the red) ascended the aorta and filled all the large vessels going to the head and upper extremities. The injection in all these vessels had not the slightest tinge of yellow.”

The yellow “filled the right auricle, free from admixture, except slightly at the posterior part of the auricle, as already mentioned. From the right auricle it filled the right ventricle, passed along the pulmonary artery, and filled the *ductus arteriosus* and branches, going to the lungs. On entering the aorta it passed down that vessel, filling it completely, without any admixture of red; and thus, all the branches of the thoracic and abdominal aorta were filled with yellow. The whole of the red had passed to the upper part of the body,” &c.

Dr. Reid's experiments have, however, entirely failed to convince us that the Eustachian valve does, or can, turn the current of blood from the inferior vena cava, across the right auricle, and through the foramen ovale, into the left auricle, without admixture with the venous blood from the inferior cava.

It is to be remarked, in the first place, that the complete isolation of the red and the yellow injections occurred only once in three similar experiments, and then in a foetus of seven months, the others being of four months, and at full term. Dr. Reid accounts for failure in the last case, since “the Eustachian valve is supposed to be less perfect at the full time than at an earlier period.” What Dr. R. supposed, is really the fact, but would it be so, *if* the valve perform the part assigned to it? On the contrary, we should suppose the necessity for the separation of the venous from the arterial blood would increase (if such necessity at any time exists) as the foetus becomes farther developed; and, therefore, that the valve would be most perfect just at the very end of intra-uterine life. It will, however, be shown that no such necessity actually exists at any time during fetal existence.

Again, we cannot suppose two currents of injection, forced through the venæ cavæ of a dead foetus, would comport themselves precisely as would the two currents of blood during life; even were the heart a passive organ. But when we consider its cavities, as exerting both a forcing and a suction power, the difference is vastly increased. In this view, the experiment

* Edinburgh Med. and Surgical Journal, Vol. 43, p. 11, &c.

appears utterly valueless, therefore. But if we are still inclined to regard the result as reliable, then it proves altogether too much—such a perfect non-admixture of the two currents during life being altogether inconceivable.

A distinguished anatomist remarks, in regard to the assumed crossing of the two blood-currents in the right auricle, without intermixture: "how this crossing is affected, the theorist will wonder; not so the practical anatomist."* He then speaks of the direction of entrance of the two *venæ cavæ*, of the opening of the inferior almost directly into the left auricle; and of the use of the Eustachian valve—as enabling the anatomist at once to perceive the correctness of this theory. We confess, that, in our own case, the theory is more satisfactory without the anatomy than with it. Or, perhaps, we should rather say, that if our mere anatomy admits the belief in such a non-admixture in a dead fœtus, our physiology will not for a moment, in a living one. And if there is an admixture, the assumed function of the Eustachian valve must be relinquished and another be assigned to it, if possible.

On merely *anatomical* grounds, the anterior termination in the auricle of the superior cava may be regarded as necessitated by the relations of its primitive branches, and, therefore, of its trunk; the former being in front of the arteries rising from the arch of the aorta, while the termination farther back in the auricle of the inferior vena cava is demanded, by the liver being in front of it. Thus, also, its termination in the right auricle, so near the foramen ovale, may be necessitated by its relations to the aorta, and to the middle line of the trunk before perforating the auricle; and, therefore, the Eustachian valve may be considered as having a more important relation to the opening of this same vein, and the passage of blood through it, than to the circulation through the foramen ovale—though it may incidentally exert some influence on the latter.

These suggestions, be it observed, are made on merely anatomical grounds. They are made to meet the inferences just objected to, and to show that the latter alone cannot form a reliable basis for the physiology of the fœtal circulation. Other facts, however, will be adduced in favor of the inferences just made; and the anatomy itself will receive some important corrections as we proceed. Meantime, we will regard the assumed function of the Eustachian valve as not proved, and as improbable.

Objections to the physiological views generally entertained.

1. No importance is attached to the fact that most of the blood from the placenta circulates directly through the liver; except so far as its influence upon the development of the liver itself is concerned, and so far as the

* Wilson's Anatomy, p. 554.

liver is a depurating organ. Indeed, it is incorrectly assumed, that the major part of the blood passes through the ductus venosus into the vena cava inferior.

2. The fact that venous blood is returned from the lungs through the pulmonary veins to the left auricle, is entirely overlooked; it being assumed, also, that almost no blood enters the lungs at all,—since,

3. It is incorrectly stated that almost all the blood in the trunk of the pulmonary artery passes into the descending aorta through the ductus arteriosus.

4. No stress is put upon the dilatation or the contraction of the auricles, as having any influence in mixing the blood contained in the two auricles, through the foramen ovale.

5. It is assumed that the greater development of the head and upper extremities at birth, is owing to their having received a very pure blood during foetal life; and greatly different in this respect from that sent to the trunk and lower extremities.

The validity of these objections, and others hereafter to be suggested, will appear in a subsequent part of this paper. Some remarks, however, in regard to the relative size of the ductus venosus and the umbilical vein, and of the ductus arteriosus and pulmonary artery, are proper in this connection.

I. *Relative size of ductus venosus and umbilical vein.*

To settle this question, I carefully dissected foetuses of from six months to eight and a half months. They were not injected, from the fact that injection passes more easily through the ductus than through the liver, in the dead foetus; and, therefore, the former becomes enormously distended, while the umbilical veins, and especially those in the substance of the liver, are less so. Hence it is impossible, in this way, to get a true idea of the relative size of these vessels. I have an injected foetus at full term, in which the ductus venosus is $\frac{22}{100}$ inch (.22) in diameter, but contracting to $\frac{10}{100}$ inch (.1) just before entering the vena cava; while the umbilical vein is only $\frac{25}{100}$ inch (.25) in diameter, and the inferior cava is considerably *smaller* than the ductus. Each umbilical artery, moreover, is as large as the aorta actually is in the living foetus at full term. It is from such exaggerations that we have heretofore acquired our ideas of the relative size of these vessels.

To avoid this source of error, I carefully dissected these vessels, and completely emptying them, laid them in their collapsed state upon a plane surface, expanded to their fullest width by the required manipulations and pressure. The width thus obtained equals the semi-circumference, or $\frac{1}{2}$ of the diameter. The results are not absolutely accurate, for reasons at once apparent. But when vessels of the same structure are thus compared (as veins with

veins and arteries with arteries), the relative size is thus more nearly obtained than in any other way which has occurred to me.

In a fetus of about eight and a half months, I found the ductus venosus to have a semi-circumference of less than $\frac{1}{8}$ inch (.05); and the umbilical vein of $\frac{1}{8}$ inch (.15). In a fetus of six months, the former had a semi-circumference of $\frac{1}{8}$ inch (.04), and the latter, of $\frac{1}{8}$ inch (.12)—the proportion being about one to three in both cases. Each of the three hepatic veins is quite as large as the ductus venosus; and these, with the latter, carry into the vena cava inferior all the blood brought from the placenta by the umbilical vein, and from the formative branches of the vena portæ. (Fig. 2d, 7.) Admitting that one of the hepatic veins is sufficient to transmit the last mentioned blood (and the small size of the vena portæ before entering the liver warrants this supposition) we have two hepatic veins and the somewhat smaller ductus venosus, to transmit to the vena cava *all* the blood from the placenta; and can perceive no adequate reason why the ductus should transmit more than one-third of that blood. Anatomy leads to this inference; though anatomical *experiments*—injected preparations—have led to a different conclusion. We consider the former far the more reliable. In the adult, the blood passes through the pulmonary artery and its branches, and through the capillaries of the lungs, and thence through the pulmonary veins into the left auricle—just as rapidly as it passes through the aorta, and just as rapidly as it could pass through the trunk of the pulmonary artery if it terminated at once in the aorta. Probably the same obtains in the case of the foetal liver. The living blood passes rapidly through its capillaries, because nutrient changes are there going on; while an injection is first stopped by these minute vessels, then flows back through the large trunks to distend them, and finally distends to the utmost, the ductus venosus; and thus at last finds its way almost entirely through this passage into the inferior vena cava and the right auricle.

Therefore, it is believed to be within the limits of truth to say that not over one-third, at most, of the blood returned from the placenta through the umbilical vein, is carried into the inferior vena cava by the ductus venosus.

II. *Relative size of ductus arteriosus and pulmonary arteries.*

In a fetus of six months, I found the semi-circumference of the pulmonary artery, before its division, to be $\frac{1}{8}$ inch (.12) of an inch; and that of the ductus arteriosus, $\frac{1}{8}$ inch (.04). In a fetus of eight and a half months, the semi-circumference of the pulmonary artery was $\frac{1}{4}$ inch (.25) inch; and of the ductus arteriosus, $\frac{1}{8}$ inch (.08) inch. In both cases the ductus was perceptibly smaller than either of the two branches of the pulmonary artery, sent to the lungs. In the older fetus it was also somewhat smaller than either carotid artery; in the youngest I omitted to notice this comparison. In these cases,

I measured the vessels in a collapsed state, as before described. The ductus tapered towards its distal extremity, in the older fœtus; and was somewhat smaller at its union with the descending aorta, than the dimensions above given.

The *anatomical* inference is, therefore, that the ductus arteriosus does not transmit any more blood into the descending aorta than each branch of the pulmonary artery carries to its lung. If it be remarked that the blood may rush more rapidly through the ductus than through the pulmonary arteries, and therefore a greater quantity be discharged, I reply that it cannot be transmitted in a current more rapid than that in the descending aorta, with which it blends; and as the latter cannot be more rapid than that in the main trunk of the pulmonary artery (since the latter and the aorta, at their commencement, transmit the same amount of blood, as nearly as may be), it follows that the current through the ductus is not more rapid than that through the pulmonary artery; and the ductus being one of its three branches, it is doubtless far less so. But, it will be remembered, that the two branches of the pulmonary artery, in the adult, transmit as much blood as the aorta, and, of course, with a rapid motion. Is there any assignable reason why the rapidity is not as great in the lungs of the fœtus as in the adult? or, therefore, why they will not transmit as much blood, in proportion to their capacity, and thus carry two-thirds of that received by the pulmonary trunk, while the ductus arteriosus receives but one-third of the same? We think not. But if it should hereafter be proved that the ductus carries even one-half of the blood in the pulmonary trunk, into the descending aorta, from having a more rapid current than the two pulmonary branches, the relative size of the ductus and the pulmonary artery remains as before stated; and it will appear that the physiology of the circulation is the same, whether the proportion is greater or less within these limits.

The relative size of the *umbilical arteries* is also greatly exaggerated in injected preparations. Each of these is actually a little more than one-half the diameter of the umbilical vein; and once and one-half the diameter of the external iliac artery; still being hardly larger than the carotids. When, therefore, we consider how many branches have been previously given off, from the arch to the descending aorta, it cannot be supposed that more than one-fourth, and probably not over one-eighth, of all the blood sent by the left ventricle into the aorta, can be by them returned to the placenta. Only the same amount is, of course, returned from the latter by the umbilical vein; and the mistake of supposing all the blood of the fœtus to be transmitted rapidly through the placenta, as that of the adult is through the lungs, is thus corrected; for the fœtus has a *reptile* circulation.

The accompanying figures will explain the preceding statements.

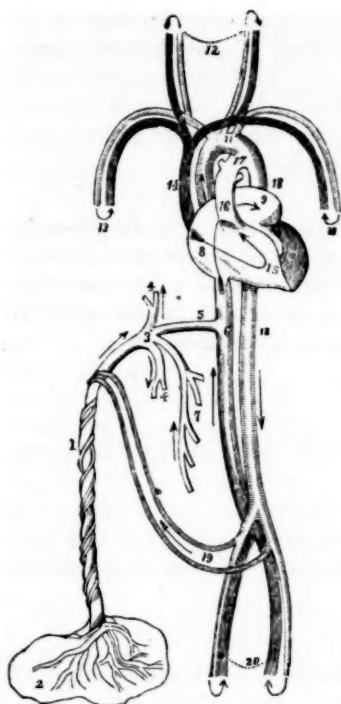


Fig. I.

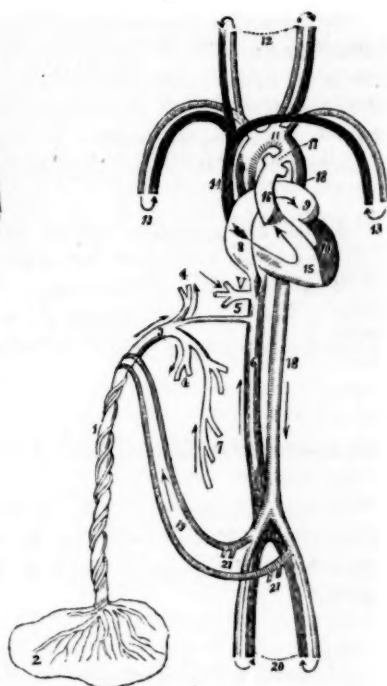


Fig. II.

Fig. I.—Diagram representing the anatomy and physiology of the foetal circulation—from Carpenter, and Wilson. The numbers represent the same parts as those in the next figure.

Fig. II.—Illustrating the anatomy and physiology of the foetal circulation—as demonstrated by the writer's dissections.

1—The umbilical cord, with its vein and two arteries, proceeding from 2, the placenta. 3—The umbilical vein, giving off, 4, 4, two principal branches entering 7, the vena portæ; and a third, 5, the ductus venosus (here of its natural size, as compared with 3 and 4), which enters 6, the inferior vena cava. V—The hepatic veins receiving all the blood carried into the liver by 4, and 7; and discharging it into the inferior vena cava above the ductus venosus. The three branches generally terminate separately in the vena cava. 6—The inferior vena cava. 7—Formative branches of vena portæ, from alimentary canal, &c. 8—The right auricle; the arrow indicating the course of the blood thence into 9, the left auricle. 10—The left ventricle. 11—The aorta. 12, 13—Arrows, showing the return of blood from the head and superior extremities to 14, the superior vena cava; and thence into 8, the right auricle, and 15, the right ventricle; and, in the course of the bent arrow, from the latter into 16, the pulmonary artery. 17—The ductus arteriosus (actual size) terminating in 18, 18, the descending aorta. 19—The umbilical arteries (continuations of the internal iliac, and larger than the external iliac) returning a part of the blood in the descending aorta to 2, the placenta. 20—The external iliac arteries, distributed to the lower extremities; the arrows at their extremities show the return of venous blood, by the iliac veins, to the inferior vena cava. 21—The remaining branches of the internal iliac (besides the umbilical arteries), and which are distributed, as in the adult, to the organs in the pelvis, &c.

Fig. 1st is taken from Carpenter's Physiology, and is here introduced for the sake of contrast with the correct representation. Fig. 2d shows the relative size of the ductus venosus and umbilical vein; of the ductus arteriosus and the pulmonary arteries; and of the umbilical arteries and umbilical veins, &c. It also shows the trunks of the hepatic veins, hitherto omitted; and some branches of the internal iliac, to the pelvis.

I shall now proceed to examine particularly each of the six physiological propositions which have been given as embodying the received view of the fetal circulation; in the course of which examination still other objections to this view will be suggested; and, finally, such conclusions will be deduced as the present state of physiological science demands.

Examination of the received view of the Physiology of the Fœtal Circulation.

In this examination I shall separately discuss the six propositions on pages 322 and 323, and will refer the reader to those pages to avoid repetition here. The passages which are italicized will demand a particular notice.

Proposition 1st.—Doubtless the placenta is a “depurating” organ, in its influence upon the blood of the fœtus: *i. e.*, the blood, while traversing its minutest vessels, parts with some of its carbonic acid gas, and receives instead, from the mother's blood circulating in the maternal portion of the placenta, an amount of oxygen gas. Thus the blood becomes aerated, and in the adult would properly be termed arterial blood. This pure blood is, however, not found unmixed in the arteries of the fœtus as it is in the adult, and therefore I shall term it instead, *placental* blood, to avoid all misunderstanding. The *arterial* blood, *i. e.*, the blood in the arteries of the fœtus, is a mixture of *placental* and *venous* blood, as will be shown.

But of the pure *placental* blood, not more than one-third, as has been seen, at once enters the inferior vena cava through the ductus venosus; the remaining two-thirds, or more, being distributed to the liver through the vena porte. The former one-third, blending with the venous blood in the inferior vena cava (at least two or three times as much in quantity) is then carried as *mixed* blood (venous and placental, and probably considerably more than two-thirds *venous*) into the right auricle, where we leave it for the present. The remaining two-thirds of placental blood are, 1st, blended

with the venous blood brought to the liver from the formative branches of the vena portæ (fig. 2—7), which may be equal to one-third the placental blood in the vena portæ; and thus this also becomes mixed blood, and three-fourths placental. But from this mixed blood, the liver is nourished; and thus, according to all analogy, it becomes entirely venous blood, on emerging from the capillaries of the liver into the radicles of the hepatic veins. This blood must, therefore, enter the right auricle from the inferior cava as venous blood; and thus the pure placental blood, passing through the ductus venosus, has been mixed with *five times* as much* venous blood, at least, by the time it has arrived in the right auricle; and the mixture is now but one-sixth placental, and five-sixths venous blood. This, also, will be the blood to enter the left auricle, provided no further admixture previously obtains.

But it is also assumed that the foetal liver, as well as the placenta, is a "depurating" organ; and, therefore, the blood discharged by the hepatic veins is arterial (*i. e.*, aerated) blood, as well as that transmitted by the ductus venosus. By a *depurating* organ must here obviously be meant an organ that renders impure or venous blood arterial; or that eliminates carbonic acid gas from it, and replaces the same by oxygen. No one, however, it is believed, will distinctly assert that the liver has any such function as this.

It is well known that in the adult, the liver is an eliminator of carbon from the blood, and is therefore, in this respect, a depurating organ. It is, however, such, only so far as it secretes bile; and in doing this, it does *not* convert venous into arterial blood. We may, therefore, admit that in the foetus also, the liver separates carbon from the blood, so far as it secretes bile; while, at the same time, there is no reason to believe that it can possibly in any degree convert venous into arterial blood; or, much less, render placental blood more highly arterialized.

To what extent, therefore, the liver secretes bile in the foetus, is an important inquiry in this connection. In the adult, bile is secreted for two entirely distinct objects,—1st, to aid the process of digestion; and, 2d, to separate, at the same time, certain hydro-carbon compounds from the blood, as a depurating organ, in the sense last specified. These impurities are separated from *venous* blood; and such as, being collected by the vena portæ from the alimentary canal and its appendages below the diaphragm, contains the crude elements of the food, abounding in the hydro-carbon compounds alluded to.

* Twice as much in inferior vena cava; *as much* from branches (7) of vena portæ and twice as much from two hepatic veins.

On the other hand, in the foetus, the bile cannot be secreted in aid of digestion at all. Moreover, the blood from the alimentary canal does not here differ from venous blood in other parts of the foetus; it being nowhere laden with the hydro-carbon compounds derived, in the adult, from the food. Very little bile, then, it may be supposed, is separated as an impurity from this venous blood in the formative branches of the vena portæ, though a very small quantity may possibly be there separated. But there is no other source for bile besides, except the pure placental blood in the liver from the umbilical vein; and to suppose this secretion to be separated from arterial blood, is both to adopt an idea opposed to all analogy, and, at the same time, to assume a necessity for the production of such a secretion from elements assumed to exist in the placental blood; while there is not a shadow of reason for believing either in such a necessity, or in the existence of such elements.

But we are still pointed to the fact, that the meconium found in the alimentary canal at birth contains none of the peculiar elements of bile; and to this fact we are by no means indisposed to give its due weight. It has already been admitted that the foetal liver may secrete a small amount of bile from the venous blood derived from the formative branches of the vena portæ; and the following facts go to prove that the amount actually is very small. 1. The whole quantity of meconium secreted during foetal life (none being evacuated till after birth) is small; and yet more than the last half of foetal life* is required to produce the small amount found in the intestines at birth—a fact showing how slowly it is separated from the blood. But, 2d, only a comparatively small part of the meconium is actually bile. According to Dr. Davy's analysis,† 100 pints of meconium consist of—

Water,	72.7
Mucus and epithelium scales,	23.6
Cholesterine and margarine,	0.7
Coloring and sapid matter of bile and oleine,	3.0

Here we find less than 3.7 per cent. of matter peculiar to bile, while at least 8 per cent. of such matters are found in the pure secretion. The inference, therefore, is, that less than one-half of the meconium is actually bile. Simon's analysis of *dried* meconium leads to a similar conclusion; it containing only 40 per cent. of matter peculiar to bile, while dried *bile* contains 80 per cent. of such matter.

We cannot, therefore, regard the foetal liver otherwise than as a very feeble depurating organ, though it is such in the sense last explained. But I sub-

* No meconium is found in the duodenum till the fourth month is completed.

† Simon's Chemistry of Man, Vol. II.

mit that the bile is secreted, not mainly to depurate the blood of the fœtus, but to secure the discharge of the mucus and epithelium scales from the alimentary canal immediately after birth, and thus to prepare it for the reception of food. At all events, it is believed that the conversion of the placental blood into venous, in the capillaries of the liver, as before explained, would far more than compensate for all the depurating power the liver can possess as a secretor of bile; and, therefore, that the placental blood transmitted by the hepatic veins into the vena cava does *not* enter the latter vessel in a purer state than it entered the liver.

Consequently, it does not leave the fœtal liver "in the condition of arterial blood;" and to say that, after it enters the vena cava, it "loses this character in some degree by the time it arrives at the heart," has been shown to be altogether too feeble an expression to cover the facts—five-sixths of *all* the blood entering the right auricle from the vena cava inferior being venous.*

Why, then, is the liver so enormously developed in the fœtus, if not a powerful depurator of the blood, as a secretor of bile? Because it has another entirely distinct and but recently discovered function to perform. But this question will be more appropriately answered in our remarks on the sixth proposition.

Proposition 2d.—In regard to the non-admixture of the blood from the inferior vena cava (five-sixths venous), while in the right auricle, with that entering the same cavity from the superior vena cava, certain doubts have already been expressed. * Of course, all belief in the complete non-admixture is based upon the supposed peculiar action of the Eustachian valve, which (it is said) carries it at once through the foramen ovale into the left auricle. It has been shown that Dr. Reid's experiments on this point are entirely inconclusive, and that, on anatomical grounds, it is as probable that this valve has a more important relation to the circulation through the inferior vena cava, than to that through the foramen ovale. But such as still insist on this non-admixture, are requested to explain how, by any *possibility*, the two auricles can contract from 140 to 150 times a minute (as in the fœtus) without securing some admixture, through the foramen ovale, of the blood

* This proposition is adopted, it will be remembered, on the calculation that the ductus venosus carries one-third of all the placental blood into the vena cava inferior. But if even *one-half* be assigned to this duct, and no allowance at all be made for the blood carried into the liver by the formative branches of the vena porta, it would still follow that three-fourths of all the blood poured into the right auricle from the inferior cava is venous blood. No one, it is believed, can demand such an assignment, however, with a knowledge of the facts I have stated, or will doubt that five-sixths is a more accurate calculation.

distending the two auricles? And if this result does occur at all, then the object of the supposed non-admixture of the two currents in the right auricle is just so far frustrated; and it is hardly probable that a special mechanism is provided to keep the blood from mixing when it first arrives in the right auricle, and another which secures its admixture the $\frac{1}{15}$ th part of a minute afterwards. Or, if it be remarked that the *valve* of the foramen ovale prevents the mixture of the blood *through* the foramen, we will recur to the period of foetal life, when there was no septum at all between the auricles, and the subsequent period when the foramen exists, but no valve is yet formed. Must there not be a *complete* admixture of all the blood in the auricles up to this time? And must not this admixture through the foramen continue, only diminishing in proportion as the valve of the foramen becomes more complete.

In a fetus of somewhat more than six months, I find the valve occludes near three-fourths of the foramen, and in one of eight and a half months, about five-sixths of it. It will, therefore, admit still of a considerable degree of mixing of blood through it; and not until the foramen is completely closed (which, of course, never occurs till after birth), can all admixture through it be prevented. But this partial prevention of admixture is not the object of the development of the valve of the foramen, but merely incidental to its formation. Nor is the well-being of the fetus at all compromised by such admixture, though the contrary has been hitherto affirmed. The following considerations will illustrate this point.

It has already been stated, that at one period of foetal existence, the circulation is analogous to that of a fish. At this time the heart consists, as in the fish, of a single auricle, a single ventricle, and a bulbus arteriosus. Subsequently, the ventricle is divided into two cavities, by the formation of a septum, completed at the ninth week; and now the foetal heart is, in respect to its function, precisely that of the reptile, and consists of two ventricles and a single auricle.* This is, in fact, the type of the foetal circulation till birth. The septum between the two auricles, first appearing at about three months, is for some time very imperfect, from the opening called the foramen ovale remaining very large; then the valve of the foramen ovale gradually diminishes the opening, as it increases in size, and in the same proportion interferes with the mixture of the blood in the two auricles during their contraction. But we must regard the formation of the valve merely as a gradual approximation to the type of circulation to come at once after birth, and to the construction of the true *mammal* heart—of two auricles and two ventricles—which is required by that new mode of

* The precise difference between the foetal and the reptile heart in *structure*, will appear on a subsequent page.

existence. We can only say, the valve, by its gradual development before birth, has, by that time, produced a *mammal heart*, as *nearly as is consistent with a reptile circulation*; and which may at once act as a mammal heart when birth takes place. The foramen ovale is perfectly closed in about eight days after birth; and then the true mammal heart becomes perfect, in structure as well as in function.

But though the valve of the foramen ovale *may*, incidentally, to some extent prevent admixture of the blood in the two auricles, while they are contracting during the last few weeks of fœtal life; still, this result is of no importance whatever, since it will now be shown that the blood in the left auricle is, on its first arrival in that cavity, probably quite as impure as that in the right auricle.

This proposition implies that the Eustachian valve does *not* prevent an intermixture in the right auricle of the blood from the inferior vena cava with that from the superior cava, by directing the former current at once from the vein through the foramen ovale—as is assumed. Objections to Dr. Reid's experiments have already been made, and others will now be added.

1st. If the Eustachian valve is formed to turn the current from the inferior vena cava at once through the foramen ovale, it must be admitted that its position is very unfavorable for the accomplishment of that object. The best position would evidently be secured, if it projected from the outer wall of the vein, and thus presented its free border towards the foramen ovale, in a line extending antero-posteriorly. According to my observations, it actually projects from the anterior wall of the vein, though obliquely, so as to terminate at the antero-inferior border of the foramen ovale. Sometimes, however, it is attached even to the middle of the posterior (or left) border of the foramen ovale; a position far more unfavorable than the one just mentioned. A glance at the following figure will illustrate this point.

2d. It has been remarked, that if the Eustachian valve does keep the purer blood from the inferior vena cava from mixing with the less pure blood from the superior cava, we should expect it would remain perfect till birth; while in fact it becomes atrophied from the seventh month, and continues to diminish, while the valve of the foramen ovale increases in size.

3d. The increase of the latter valve, while the Eustachian is diminishing, might seem to imply that the former comes, in some degree, to supply the place of the latter. But it is impossible that the former can, in any degree, direct the blood from the inferior vena cava *through the foramen ovale*.

Fig. III.—Modified from Weber. The Eustachian valve at six months. A—Inferior vena cava. B—Superior vena cava. C—Eustachian valve in front of termination of A; its inner extremity attached even to the middle of the posterior border of the foramen ovale, D. The upper end of the probe passing through the inferior vena cava, rests on the posterior wall of the right auricle; and just below it is the opening of the coronary vein, partly closed by its valve.



It is inferred, therefore, that the Eustachian valve is not formed to "prevent in great degree, if not entirely," the admixture of the blood from the inferior with that from the superior cava. That it may have some effect of this kind while the valve is largest, is possible; but this possible effect is incidental to another function, and in itself, if actual, is of no physiological importance whatever.

What, then, is the true function of the Eustachian valve? After much reflection, and study of its form and relations, the ideas of Winslow,* published nearly one hundred and fifty years ago, seem the most philosophical, viz., that it opposes the regurgitation of the blood from the right auricle into the inferior vena cava; while it also, as Lancisi maintained, prevents the current from the superior cava from falling too forcibly upon that of the cava inferior. The effects of such a regurgitation in preventing the arrival of the placental blood in the heart, need not be specified; and it is equally apparent that if this be the true function of this valve, it is replaced, so far as regurgitation, from the left auricle, and then into the inferior cava is concerned, by the valve of the foramen; and, therefore, in proportion as the latter is developed, the former may become atrophied, as already explained. It may, also, be added, that the position of the Eustachian valve, though unfavorable for directing the blood through the foramen ovale, is the only possible one in which it could both prevent regurgitation and break the impetus of the current from the superior vena cava, and, at the same time, *not much* obstruct the passage of the blood through the foramen ovale. These three results are, however, unimportant after birth, and when no placental or arterial blood is to enter the right auricle; and hence the valve is atrophied and useless after that event.

* Memoires de l'Academie Royale, 1717.

3d Proposition.—Before entering upon this, we may recapitulate the two principal points just established, viz.:

1. That of the blood arriving in the right auricle from the inferior vena cava, about one-sixth is pure placental blood, and five-sixths are venous; and,

2. That the Eustachian valve has little, if any, power (and none, certainly, in the last part of foetal life) to prevent the admixture of the blood arriving in the right auricle from the two venæ cavæ.

Now, admitting that the superior vena cava discharges but one-half as much blood as the inferior—and which is certainly a sufficiently *low* estimate—it will add three parts more of venous blood to the three of venous and one of placental blood from the inferior vena cava; and thus, of all the blood filling the right auricle, one-ninth will be placental and eight-ninths venous.* It has also been shown that the same mixed blood must enter the foramen ovale and fill the left auricle, except so far as the Eustachian valve may possibly, incidentally, and in less degree as the full term approaches, direct a somewhat larger proportion of the blood from the inferior cava through that opening, than of that from the superior cava. If, however, any difference as to the purity of the blood in the two auricles be produced in this way, it must be before the Eustachian valve is much atrophied; and, therefore, at the time when, the valve of the foramen ovale being still slightly developed, the blood in the two auricles will be mingled through the foramen ovale at each contraction. We, therefore, conceive no essential difference as to the purity of the blood in the two auricles can exist. If, however, it be still asserted that the blood would not be mixed to any considerable extent through the foramen during the last four to six weeks before birth, let it be remembered, both that the Eustachian valve cannot then perform its assumed function, and also that the blood in the left auricle is constantly rendered less pure by the venous blood entering it directly from the lungs, through the pulmonary veins. The amount of venous blood thus returned is far greater than has been hitherto admitted, being about two-thirds of all sent through the pulmonary arteries. The improbability is therefore extreme, that the blood in the left auricle is essentially, or indeed in any degree, purer than that of the right auricle. It may also be remarked that the dilatation or *diastole* of the auricles, exerting a suction power on the blood entering these cavities from the veins, would also commingle the blood as it enters the right auricle, and then passes through the foramen ovale to the left; and even before the auricular contraction takes place.

* If three-fourths instead of five-sixths be insisted on (note, p. 333), then the blood filling the right auricle would be one-sixth placental and five-sixths venous. In either case, its *highly venous* character is established.

We, therefore, perceive that when the ventricles contract, after receiving the blood from the auricles, the *left* ventricle does *not* send "arterial" blood, *before undergoing any admixture*, into the ascending aorta, &c.; but it sends a *mixed* blood, probably eight-ninths venous (and certainly more than five-sixths venous) into that vessel. Neither does the *right* ventricle send more *venous* blood into the pulmonary artery and onwards; the latter blood being as pure as the former, or five-sixths to eight-ninths venous.

The same is true of the reptile, though the heart of the latter consists of two auricles and but a single ventricle. One auricle receives the venous blood from the whole body, like the right auricle of the human adult; the other receives the aerated blood from the lungs. Both contracting simultaneously, these two kinds of blood are forced into the single ventricle, and there mixed; and the latter sends this *mixed* blood through the aorta to the body generally, and also through another much smaller vessel—the pulmonary artery—to the lungs, for further aeration; equally pure blood being sent through both these vessels.

The fœtus has a *single auricle* (*actually* at the third month and later, and *practically* until birth), and two ventricles. Yet, precisely as in the reptile, the same *mixed* blood is poured into the aorta and the pulmonary artery; it being mixed, in the reptile, in the single ventricle, and, in the fœtus, in the (practically) single auricle, before it enters the double ventricle. Why, then, is not the auricle of the fœtus double and the ventricle single, as in the reptile? A little reflection will show that the arrangement actually existing in the fœtus, is the only simple one which is compatible with the requirements of the case, viz., a *temporary reptile circulation to be instantly changed to a permanent mammal circulation*.

What has so long been taught, therefore, in regard to the ductus arteriosus carrying *venous* blood into the descending aorta for a particular reason, must at length fall to the ground.

4th Proposition. It does *not*, therefore, follow that the "head and superior extremities are supplied with blood *nearly as pure as that from the placenta*;" though it is true that these parts are developed in advance of the trunk and lower extremities. Nor is it the fact that the last mentioned parts are supplied with a less pure blood than the others. It has been shown that the pulmonary artery receives *as pure* blood as the aorta. This is necessitated, also, from the physiological fact that the pulmonary artery is the *nutrient* artery of the lungs;* as the history of the development of

* Dr. Heale has recently demonstrated the same proposition; and shown that the bronchial arteries only give the vasa vasorum to the pulmonary, and supply the pleura, also, in part. See April ('54) No. of the MONTHLY, p. 302.

these organs clearly indicates; though it would carry me too far from my present purpose to explain this point at length. It has also been shown that the blood entering the aorta from the left ventricle is as impure as that contained in the right side of the heart. If still farther proof is required, it may be remembered that in cases of cyanosis, or patency of the foramen ovale after birth, the livid color of the face, and all other parts of the surface, affords a visual demonstration of the assertion that the aorta circulates highly *venous* blood. And can we suppose the blood in the aorta is purer before birth than afterwards?

It has been assumed that the earlier development of the head and superior extremities, is at the same time an effect of their being nourished by a purer blood, and also a proof of such superior purity—a good illustration of what logicians term “reasoning in a circle.” It has been shown that no such difference in the purity of the blood can exist. But this question will also be briefly considered, on independent grounds, since so much importance has been attached to this assumption.

It is true that the organs of sensation, deglutition, and prehension, are early needed and early developed; and this implies an early development of the brain and spinal cord—and, in a word, of the head and superior extremities. These parts are also supplied with blood from the arch of the aorta. But, it is equally true, that the alimentary canal, and the urinary apparatus, are quite as far developed at birth; though the latter organs are all supplied by the descending aorta. A difference in the purity of the nutrient blood cannot, therefore, account for the more early development. Or, if it still be insisted that we may thus account for the facts, how then shall blood of *equal* purity, sent to the upper and lower extremities *after* birth, enable the latter to gain upon and overtake the advanced development of the former?

And, again, if a purer nutrient blood produces a more rapid development of the head and superior extremities of the human foetus, the same should obtain in all the mammalia—the foetal circulation being in all essentially the same; and we may inquire, why the posterior extremities of the calf, the sheep, and the dog, are, at birth, equally developed with the anterior extremities and the head?

Thus comparative physiology again leads us to the conclusion that there is no difference as to the purity of the blood contained in the ascending and the descending aorta, as it previously has indicated that the blood in the pulmonary artery and the ascending aorta are, in respect to purity, the same.

How, then, shall we account for the earlier development of the head and superior extremities? We can only say it is an ultimate fact—a *law of development*. And yet, it is certainly no more difficult to explain how they are *first* developed while they receive the *same* blood as the lower extremi-

ties, than it would be to explain why the alimentary canal and kidneys are *equally* developed with them, on the other supposition—that the latter are nourished by an *inferior* blood.

The great law of development appears to be, that “the parts and organs first needed are first developed;” and is applicable also to different parts of the same apparatus, and even to different portions of the same organ, e. g., the ribs are developed earlier than the sternum—the bones at the base of the cranium earlier than those of the vertex—the laminae of the vertebrae before the bodies, and those of the dorsal region first of all—and the organ of sight previously to that of smell. But, as the parts and organs needed, immediately after birth, vary in different animals, we find a corresponding difference in development up to that period. The young of the marsupialia, remaining, for some time after birth, in a pouch, and attached to the mamma of the mother, is an embryo, rather than a fetus, when born, so far as development is concerned. The herbivora and carnivora, needing immediately to be able to stand and walk, have all the four extremities equally developed at birth. The human infant is very differently circumstanced in this latter respect; and to distend the uterus during gestation with a pair of well-developed inferior extremities, which are not to become available for several months after birth, would certainly be productive of some inconvenience, without any compensating advantage; especially since, as things now are even, the motor powers of these less-developed limbs are not seldom by mothers found to be inconveniently energetic.

Thus, the idea of a purer blood being sent to the head and upper extremities, is shown to be untenable, from whatever point of view we consider it. And, as it was suggested at first, from a mere desire to account, and for the special purpose of accounting, for the earlier development of the upper parts of the fetus—and other facts have been perverted to support it—it is, at last, high time to remand it back to the brain, now long since mouldering in dust, which first conceived it.

5th Proposition.—We have next to show the incorrectness of the assertion, that “in the fetus the lungs are solid and *almost impervious*,” and that the “*greater part*” of the blood in the pulmonary artery “rushes through the ductus arteriosus” into the descending aorta.

It is true that the fetal lungs are more solid than they are after birth, but no more so than the liver or the kidneys; and therefore we discover no reason why they are more impervious to the circulation than these organs—which transmit an abundance of blood. Moreover, we have discovered a reason for admitting a free circulation through the fetal lungs, in the comparatively large size of the pulmonary arteries and veins; and a *necessity* for such a circulation, since the pulmonary arteries are the *nutrient* arteries

of the lungs. It has been seen that, if we may judge from the size of the two branches of the pulmonary artery, compared with that of the ductus arteriosus, about two-thirds of all the blood sent from the right ventricle traverses the lungs; and is, of course, returned as *venous* blood to the left auricle, by the pulmonary veins. In the fœtus, a *mixed* blood is sent through the lungs for their nutrition, for entirely venous blood will not accomplish that object; in the adult, *venous* blood is sent through the lungs first for "oxydation," or aeration; and secondly, for the nutrition of the lungs after aeration is secured.*

Thus, also, the "greater part" of the blood in the trunk of the pulmonary artery does not rush through the ductus arteriosus—only about one-third of that blood is transmitted through this duct. It has also been shown that the blood it transmits is as pure as that sent from the left auricle directly into the ascending aorta. The ductus does not, therefore, terminate in the descending aorta, in order to avoid mixing its impure blood with that sent to the head and upper extremities, but for some other reason; and we deem it unnecessary to look for it beyond the fact that the commencement of the descending aorta is the *nearest* point in that vessel which the ductus can enter. What, then, is the true function of the ductus arteriosus? It is, we conceive, merely a "waste pipe," to conduct at once into the aorta all that part of the blood in the trunk of the pulmonary artery which the collapsed fœtal lungs cannot receive. Hence, as soon as the lungs are distended by the first inspiration, and thus made capable of receiving and transmitting *all* the blood sent from the right auricle and ventricle into the trunk of the pulmonary artery, the ductus becomes at once useless, and, together with the foramen ovale and the ductus venosus, becomes completely closed about eight days after birth.†

But while the fact is insisted on, that the pulmonary artery and the aorta carry equally pure blood, it must be remembered that in both these vessels there is not more than one part of pure placental blood to six or eight of venous blood. Thus the blood in the arteries of the fœtus is but slightly more pure than the venous blood of the adult. It seems to have been taken for granted that the organs of the fœtus (the head and upper extremities, at least) must be developed from pure arterial blood—blood as pure as the arterial blood of the adult. It is now apparent that no such blood exists in any artery possessed by the fœtus; but only in the umbilical

* For an explanation of this, see the MONTHLY, April No., 1854, p. 303.

† Billard found that of nineteen infants who had lived but one day, the foramen ovale was completely open in fourteen; in two, it had begun to close; and in two it was completely shut. Of twenty who had died on the eighth day, five only still had the foramen open.—*Traité des Maladies des Enfants nouveau-nés*, 1828.

vein, and the ductus venosus. All the organs of the reptile are also developed from a mixed blood.

But while blood of equal purity is sent through the pulmonary artery and the aorta in the human fetus, the blood returned to the placenta for farther purification is just as *pure* also. The umbilical arteries are merely continuations of the internal iliac arteries, and, of course, contain the same blood as the aorta. But here, again, the analogy to the reptile is perfect; the *mixed* blood in the single ventricle being sent, in part, through the pulmonary artery and lungs for farther aeration, while the rest is sent through the aorta and its branches, for the nutrition of the body generally. In the reptile, moreover, the pulmonary artery is smaller than the aorta, and less blood passes through the lungs than through the aorta and to the tissues generally. In the fetus, also, much more blood passes through the aorta than to the *placenta*—it having been shown that the umbilical arteries cannot transmit more than one-sixth of all the blood received by the aorta at its commencement. In the human adult, as much blood is sent through the lungs as through the aorta and its divisions; and it is calculated that the whole mass in the body may pass through them, and therefore become aerated in the space of three minutes; the standard of aeration in the adult is therefore high. In the fetus, on the other hand, the umbilical arteries can return the blood to the placenta only about one-sixth as rapidly in proportion as it is circulated through the lungs in the adult; and, therefore, the standard of aeration in the fetus, as in the reptile, is low. And the *mixed* blood in the umbilical arteries being returned as pure *placental* blood by the umbilical vein, this low standard of aeration of the mass of blood in the arterial system of the fetus is thus constantly maintained, though the quantity of *pure* blood returned to the heart through the ductus venosus is so small.

There are other peculiarities of arterial fetal blood, besides its *venous* character and appearance, as (compared with the mother's blood) its abundance of colored corpuscles, and its deficiency of fibrine. But, upon these peculiarities I need not dwell; nor is farther proof required, it is believed, of the incorrectness of the proposition now under consideration. And should it, at any time, be proved that even one-half of the blood in the trunk of the pulmonary artery traverses the ductus arteriosus, its physiological inaccuracy would remain unchanged.

6th Proposition.—There can be no doubt that the large size of the fetal liver is the direct result of the distribution to it of a large amount of the pure placental blood. Indeed, it has been shown that while all other organs in the fetus are nourished from a mixed blood (not more than one-sixth placental) this organ alone is nourished by a mixture of three-fourths placental blood. A comparatively early and enormous development might reasonably be expected, therefore; and it is found actually to obtain.

But we cannot regard this enormous development as merely a casual result of the distribution of a great amount of pure blood to the fetal liver, as the quotation constituting the proposition now under consideration seems to imply. We believe the law of development, before mentioned, is as applicable here as elsewhere; and that the blood is sent to the liver on purpose to develop it—it being largely and early developed, because early needed, in the fetal economy.

What, then, is the function of the fetal liver requiring so early and so enormous a development? The liver of the reptile is also largely developed, and here we discover another analogy with the human fetus. We have also seen, that it cannot be as a secreter of bile that the liver is so early needed. Bile is not found in the intestine at all till the fifth month; and it has been shown that only a small quantity, in the aggregate, can have been secreted previously to birth. On the other hand, the liver is one of the first organs becoming distinct in the embryo, constituting, at from three to five weeks, over one half the weight of the entire embryo; almost entirely filling the abdomen at twelve weeks; and constituting $\frac{1}{16}$ part of the weight of the whole body at birth. The inference is, therefore, that the liver performs some function before it becomes a secreter of bile, to a perceptible extent—that this is a more important function than the one later manifested—and that it continues in full activity till birth, when it diminishes.

Those who are familiar with the recent experiments of Bernard, of Paris, will not hesitate to decide, that the function of the liver, as a *blood-making* organ, is the one in question. For, it must not be forgotten, that the fetus makes its own blood; not deriving a particle of *blood* from the mother's vessels—but merely the *elements* from which it forms its own vital fluid. That the liver is the organ most important in the blood-making process, has been demonstrated by Bernard; and, surely, there is no time when its action is so much required as when the first blood is to be formed, and increased, and all the tissues are to be developed—from elements obtained from the blood of the mother. Hence the large development of this organ in the fetus.

But the liver actually diminishes in size at once after birth—the diminution affecting principally the left lobe; and which, in the fetus, is about as large as the right. Indeed, it is not till the infant is from ten to twelve months old, that the liver has again become as large as it was at birth. Doubtless the withdrawal of the pure placental blood from this organ at birth, and the substitution of mere venous blood in the vena portæ, is the immediate cause of this atrophied condition of the organ for a time. Still we cannot regard this condition as merely accidentally owing to that cause. The blood of the new-born infant is to be formed from elements contained in food, and not derived, as before, directly from the mother's blood-current. True, the milk naturally destined for its nourishment, is also obtained from

the mother's blood. But the gland secreting it has elaborated its elements from the blood; and, it being digested before entering the liver, less of the peculiar action of the latter organ is probably required in the blood-making process. When, however, the child begins to take a variety of aliment—or when somewhat more than a year old—the liver is found to have regained its weight at birth, and now gradually proceeds to its full development in the adult, when it constitutes about $\frac{1}{36}$ part of the entire weight of the body. These remarks are, however, made, not so much to explain the atrophy of the liver after birth, but as naturally suggested by and associated with its occurrence.

The vena portæ in the foetal liver is, therefore, in all respects, an *artery*; it carries the pure blood from the placenta, and is the *nutrient* artery of the liver, as the pulmonary is of the lungs. It is, however, also, something more. The formative branches constitute the true vena portæ in the foetus, as well as in the adult; and, therefore, its branches in the substance of the liver, containing the venous blood from these—mixed with the placental blood, as before explained—represent the vena portæ in the liver of the adult, containing only venous blood, from which the bile is secreted—at the same time that it is also the nutrient artery of this organ. After birth, the hepatic artery, which, in the foetus, does hardly more than afford the vasa vasorum to the vena portæ, becomes the sole nutrient artery of this organ, as it alone transmits arterial blood. For the assumption appears well founded, that after birth the tissues can be developed and repaired from arterial, or completely aerated, blood alone; while the tissues of the foetus are formed, as has been proved, from a mixed blood, of which (except, in case of the liver alone) the venous forms from five to eight times as large a proportion as the placental or aerated blood.

Having completed my examination of the propositions under consideration, I now conclude with a recapitulation of the conclusions which have been arrived at, and which together constitute, it is believed, such a view of the foetal circulation, during the last half of foetal life, as the present state of physiological science demands.

The view of the foetal circulation required by the present state of physiological science.

1st. The human foetus, during the last half of foetal existence, has a *reptile* circulation—the mammal circulation commencing at birth; and the structure and the function of each particular part of its circulatory apparatus are in subservience to this fundamental fact. The characteristics of a reptile circulation are—1, the circulation of a *mixed* blood (and of the same degree of impurity) through both the aorta to the tissues, and through the

pulmonary artery to the lungs; and 2, the transmission of far less blood to the aerating apparatus than is sent through the aorta.

2. The foramen ovale with its valve is the only simple mechanism which could answer the requirements of the case, viz., a temporary reptile circulation with a capability of instantaneous change to a permanent mammal circulation, the foramen becoming permanently closed about eight days after birth.

3. The ductus arteriosus is merely a "waste pipe," conducting into the nearest portion of the aorta that part of the blood sent into the trunk of the pulmonary artery, which the collapsed lungs of the fœtus are unable to receive. After birth the latter admit all the blood, and the ductus is, therefore, useless. It does not enter the descending aorta to avoid sending its blood to the head and upper extremities.

4. Though the lungs are more solid in the fœtus than after birth, they are probably permeated by about two-thirds of the blood entering the trunk of the pulmonary artery, and this is returned as *venous* blood to the left auricle.

5. The blood arriving in the right auricle from the two *venæ cavæ* is completely intermixed by the diastole and systole of this cavity; and the same mixed blood is therefore transmitted through the foramen ovale into the left auricle. Or, if by any possibility more placental blood enters that cavity, the venous blood returned by the pulmonary veins most probably counterbalances that advantage.

6. The Eustachian valve cannot prevent the admixture of the blood from the *venæ cavæ*, nor direct that from the inferior cava at once through the foramen ovale; it merely prevents regurgitation from the auricle into the inferior vena cava, at the same time incidentally preventing the current from the superior cava from impinging so forcibly upon that of the inferior. Hence the valve of the foramen ovale replaces it to some extent, in respect to its principal function; and, therefore, it becomes atrophied in proportion as the latter is developed.

7. No artery in the body of the fœtus contains *arterial* blood. The aorta and pulmonary artery, and all their branches, contain a *mixed* blood, about five parts, at least, venous to one part placental. The precise proportions, however, are unimportant, the blood being of a *highly venous* character, and as impure in the aorta as in the pulmonary artery. Only the umbilical vein and the ductus venosus contain pure aerated placental blood.

8. The umbilical arteries contain the same mixed blood as the aorta, and possibly return one-sixth of the blood received by that vessel; but this amount, aerated in the placenta and returned by the umbilical veins, suffices to maintain the low standard of aeration in the fœtus.

9. The head and upper extremities of the fœtus do not receive a purer

blood than the lower parts of the body. They, as well as the digestive and urinary apparatus, are earlier developed, in accordance with a general law of development.

10. The foetal liver is a *depurating* organ only so far as it secretes bile, and therefore, to a slight extent, though it does not thus convert venous into arterial blood. Its large development, from the placental blood abundantly distributed to it, has relation to its function as a *blood-making* and not as a *bile-secreting* organ; and this blood becomes *venous* in the capillaries and the hepatic veins, as all analogy proves.

11. The trunk of the vena portæ is, in the foetus, both the *nutrient artery* of the liver, and also corresponds to the vena portæ of the adult—its formative branches containing venous blood from which the bile in the meconium is probably secreted:

12. Anatomy, the history of development, and comparative physiology, combine to sustain the preceding propositions.

Bowdoin College, April 1, 1854.

Uterine Sympathy. By EDWARD WARREN, M.D., Edinton, N. C.

MANY writers have denied, that an impression made on the mind of a pregnant woman can produce any effect on the foetus, beyond that which may be ascribed to the operation of some physical and palpable cause. However specious and plausible their arguments may be, their inferences are opposed by an array of facts too substantial and imposing either to be set aside or passed over by those who will consider the subject thoroughly and fairly. It is possible that many ridiculous stories have been told in regard to the marks, alterations, &c., impressed on the child in utero, but these do not change the well-established fact that, in some instances, such modifications have been produced, and are being effected daily throughout the world. It is denied that the connection subsisting between the foetus and the woman is sufficiently intimate to render such a condition of things possible. Now, there might be something plausible in this objection, if it were not contradicted and nullified by the experience of all who have seen children resembling their parents both in feature, form, gait, and disposition. Surely, if "the connection" is strong enough to establish and perpetuate such a resemblance as this, it is sufficiently close and positive for the production of the other result.

The mistake which has been made by Buffon, Gerard, Parr, and others, on this subject, arises from an attempt to determine the theoretical ques-

tion, Can this thing be? instead of the practical one, Is it so? It is evident that the first question cannot be answered satisfactorily until the relations between mind and matter are fully understood and accurately defined. As the whole matter is involved in a mystery too profound for the penetration of a finite mind, of course, the connection between the intangible cause and the material result cannot be logically demonstrated. It has been for this reason that great minds have fallen into so glaring an error in regard to the influence exerted by the mind of the mother on the child in her womb. They have been unable to reason the thing out, and on that account have rejected it as impossible. The practical question, Is this thing so? can be more readily and most satisfactorily determined. Even if it is impossible to demonstrate that such a result must necessarily be produced, or to show why and how such things do occur, we cannot deny the fact of their existence, without impeaching the testimony of some of the most eminent and respectable men that have adorned and advanced the profession of medicine. Beside the mass of evidence furnished by medical men, Holy Writ itself gives an instance of this singular phenomenon. In Genesis, it is said that "Jacob took him rods of green poplar, and of the hazel and chestnut tree, and pilled white streaks in them, and made the white appear which was in the rods. And he set the rods which he had pilled before the flocks in the gutters in the watering-troughs, when the flocks came to drink, that they should conceive when they came to drink. And the flocks conceived before the rods, and brought forth cattle, ringed, streaked, speckled, and spotted." Thus the highest authority among men arrays itself against those who refuse to recognize the doctrine of uterine sympathy and maternal influence.

A number of instances in point might be mentioned in this connection, but a few will suffice for my present purpose. Millingen gives the case of a lady who, during pregnancy, was struck with the unpleasant view of leeches applied to a relation's foot. Her child was born with a leech coiled up in the act of suction on the identical spot. The same author, on the authority of Bennett, relates the following instance of this sympathetic agency of mind on matter. A woman gave birth to a child with a large cluster of globular tumors growing from the tongue, and preventing the closure of the mouth, resembling, in every particular, common grapes; and with a red excrescence from the chest, like the wattles of a turkey. On being questioned, *before* the child was shown her, she answered that whilst pregnant she had seen some grapes, which she longed most ardently for; and that she had been attacked and alarmed by a turkey cock. *Nævi materni* frequently resemble fruits; and it is a well-authenticated fact, that there exists a remarkable sympathy between them and what they represent. Some will actually assume a tinge of maturity when the fruit is ripening,

and become gradually more pale as it is going out of season. The same thing has also been said in regard to animal marks, which are not uncommon. For instance, they will present a deeper color when the animal by which they have been produced is mentioned or seen. From these facts, it appears that a mental impression is not only capable of directly reproducing itself, but also of developing a mysterious sympathy, the influence of which is felt and perpetuated so long as the organism concerned in its operations remains in existence.

The particular object of this paper is to add a few more instances to those already reported, for the purpose of contributing something towards the settlement of a question which has been so long mooted in the medical world.

There is a negro boy in this town, aged about ten years, in good health, and quite well grown, whose countenance bears a remarkable resemblance to that of a fox. The likeness is so great, that it strikes every observer at first sight, and attracts the immediate attention of all who see him. But this is not all: he walks and runs habitually on his hands and feet, like a quadruped, and is more active than most boys of his age who use their limbs in the ordinary way. He is solitary in his habits, shy in his manners, and of a cunning and roguish disposition. In almost every particular, some resemblance to a fox manifests itself; and when all the points of similitude are considered together, the likeness is most remarkable.

As soon as my attention was directed to him, I became interested in the case, and instituted inquiries in regard to the experience of his mother during her pregnancy.

From her, and other reliable sources, I learned that when in that delicate condition, her master secured a living fox, which he chained in a situation where she was compelled to see it daily during the continuance of her pregnancy.

The resemblance to the fox was plainly distinguishable when the child was born, and has continued to increase until the boy presents the appearance and peculiarities mentioned above.

Another case of a like nature came under my observation recently. A pregnant woman residing near this town saw a picture of a rabbit, with which she was exceedingly delighted. When her child was born, it was hare-lipped, and bore so striking resemblance to a rabbit, that the most casual observer could not fail to discover it at a glance. The infant attracted much attention because of this strange circumstance, and was visited and examined by many persons in the neighborhood. Among those who saw it frequently was another pregnant woman; and when she gave birth to her child, it was marked in the same way, and bore a similar resemblance.

The first child died early, the other is still alive and in good health.

I am acquainted with a young man, the first finger of whose right hand presents a very singular appearance. The end of it is devoid of every thing like a nail, save in three points, which correspond in size and position to the eyes and mouth of a snake, and presents almost an exact resemblance to the head of a serpent.

He says the account which he received from his parents and their contemporaries is, that when his mother was pregnant, a snake crawled into the house, to which she pointed in great alarm with the first finger of her right hand, and then fainted away.

I have no way of establishing this matter positively; but the young man was assured of the truth of the explanation, and I have every reason to consider it correct.

I have seen some cases in which the maculae resembled fruit, and know of others in which fish were distinctly represented. In all of these, the effect of an impression made on the mind of the mother by some external cause, could be traced and established. Thus, a woman was fond of pears, and longed intensely for them during her pregnancy; when her child was born, a small pear was pendant from its ear, and the last finger of the right hand. Another desired to indulge in eating crabs, but for some reason was unable to gratify herself in that respect. Her child had the figure of a crab distinctly marked upon it, and bears the naevus to this day.

In view of these facts, it is apparent that the duties of a mother begin even before the birth of her offspring. Since mental impressions are so easily transmitted and reproduced, it is as important that she should cultivate kindly feelings and elevated sentiments, during the existence of her pregnancy, as to adorn herself with the noblest and loveliest virtues for the imitation and improvement of her living child. She should avoid all causes of excitement, shun every source of disquietude, and labor to preserve a calm, composed, and peaceful state of mind. In a word, she should endeavor to develop in herself that temper, disposition, and intellect, which she would most desire for the being she is about to call into existence, and around whom the holiest ties, the fondest hopes, and the purest love of her heart must concentrate forever. I do not mean that she can absolutely endow her child as her affections would dictate or her judgment suggest; but it is impossible to consider the intimate union existing between the mother and the being within her bosom, and the mysterious influences which manifest themselves in that connection, without concluding that something may be done towards moulding the mind and character of the babe in utero, which will affect its subsequent destiny. History hardly presents a great name, whose talents were not directly derived from his mother, and whose character was not developed under the fostering care of

maternal influence. Nor is this all: there is scarcely a man mentioned either in modern or classic story, with whom dark and terrible deeds are associated, who did not learn his first lessons of iniquity from the frail daughter of humanity to whom he was indebted for his being. These facts are ominous, and should speak volumes of eloquent truth to the world.

I have thus endeavored to give a few facts which bear on the subject of uterine sympathy. I suppose every physician in practice has met with many similar and perhaps more interesting examples. If all would make public their experience, great benefits might result to the profession; for light would be thrown on one of the darkest problems of medical philosophy, and a question determined, immensely important in itself, and which must prove a source of difficulty and dispute until all the facts connected with it have been observed and analyzed.

Rupture of the Perineum, its causes, prevention, and cure. By AUGUSTUS K. GARDNER, A. M., M. D., Member of the National Medical Association, &c.

(Read before the N. Y. Academy of Medicine, and published by permission.)

Among all the causes of dystochia, rigidity of the perineum is perhaps the least common and the least troublesome. Notwithstanding the teachings of many of our works on midwifery, from not a small experience I am inclined to think that too great stress is placed upon this occasional cause of difficult and protracted labor. Not unfrequent are the cases, where the presenting portion remains down upon the perineum for a considerable period; but I am not prepared to admit that this arrest is always, or even generally, caused by an unusual rigidity of the soft parts. Not unfrequently it is noted that the dilatability of the perineum is such as to afford no resistance to the advance of the labor—the delay being due to an absence or cessation of the *vis a tergo*, the contraction of the womb; the bulk of the presenting part crowding through a narrow outlet; some unnatural growth or deformity of the pelvis, the coccyx, or the vagina; rigidity of the sacro-schiatic and coccygeal ligaments; from an unusually short *cord*, either naturally so, or from being wound about the child and arresting its descent; or, finally, from a delay in the rotation of the presenting part, which should bring the long diameter in correspondence with that of the inferior strait; that the pressure be not made upon the inner surface of the *tuber ischi*; or from an arrest in the rotation of the shoulders (where the head presents) corresponding to the diameter of the superior or middle strait in which it is placed.

I have not mentioned cases of unusual presentation—as of the face—where the delay at the inferior strait is evidently to be ascribed to the unusually long diameter of the presenting part, in reference to the diameter of the outlet. I mention these as some of the causes which delay the labor when the head is down upon the perineum, and when this delay may not be properly ascribed to the rigidity of the perineum.

This rigidity, when it does actually exist, may be referred to the strength of the muscles of that locality, being commonly found in females whose laborious life has given unusual development to them; sometimes it is due to the adhesions of the muscular fibre, from age, or from cicatrices of wounds, and occasionally to œdema of the surrounding parts.

In the progress of labor, this state of things, which impedes the delivery of the child, is overcome in two ways; first, by the gradual and complete dilatation of the perineum; secondly, by the tearing asunder the integuments to allow the passage. When the rent is slight, this is called a laceration of the perineum; it is in reality but a laceration of the vulva; but, when more extensive, it is denominated a rupture, more or less complete, of the perineum.

Instances have occurred of results, other than the above, happening in consequence of their abnormal condition. Instead of dilating or rupturing, the perineum is sometimes perforated, and the foetal head passing through the aperture thus formed. Sometimes, with tremendous disruption, the head of the child may be forced through the intestine, and pass through the sphincter ani.* Dr. Cheeseman, of this city, has related to me a case to which he was recently called in consultation—a primipara—where the perineum was uninjured and also the rectum untouched, but where, by almost a single pain, the vulva was rent, and the entire cellular tissue of the perineum, down to the intestine and through the sphincter ani, was ruptured and torn asunder, the perineum escaping uninjured. Moreau gives, as the causes of this accident, “too great projection of the sacro-vertebral angle; a great inclination of the abdominal strait; a want of curvature of the sacrum; a want of solidification of the articulation of the coccyx; too great size of the inferior strait, especially behind; a contraction of the pubic arch and excessive depth of the symphysis pubis; the presence of an arm or foot;” and other causes, before mentioned.

Great diversity of opinion exists among writers, in regard to the relative number of cases of this accident: Of serious rupture, requiring some operation to restore the parts to their natural condition, the statistics may be easily obtained. These cases are few in number compared with the number of cases of labor. But, in regard to lacerations, while most practitioners consider this of unfrequent occurrence, some go so far as to make a case of

* *Blundell's Lectures. Velpeau's Treatise on Midwifery.*

primiparous labor, without laceration, an unusual event. Rigby says, "The anterior margin of the perineum, called *frænulum*, is, we believe, almost invariably ruptured in every first case; but the laceration ought not to extend farther."

Meigs says, "The fourchette is a pretty firm fold of tissues, serving to unite the lower extremities of the vulva. It is said to be generally ruptured in a first labor, which I do not think is true. It is, doubtless, often broken, and no evil consequences commonly ensue from the accident." "Mr. Wilkin* had attended 4,000 cases of labor, and thought laceration, of any extent, very rare."

In nearly 900 cases that I have myself seen and attended, scarcely five have had any, even the slightest, giving way of the soft parts, that might not be denominated a fissure of the vulva. I have sometimes been surprised, after a labor was finished, when, during the passage of the head in its last stage, I have been quite confident, from the sensation to the hand supporting the perineum, that rupture, more or less, was taking place, to find, upon ocular examination, that it was the rapid dilatation or effacing of the perineum, under the hand, which was the cause of the sensation which had deceived me, and that no trace of the slightest fissure could be observed by the eye.

Dr. Simpson, in an elaborate paper has stated, in reference to the cervix uteri and perineum, "that fissuring and laceration are not, as has generally been conceived, rare lesions during labor; on the contrary, they are of very common occurrence; especially in primiparous labors." It is not the object of this paper to enter upon the question; and I shall merely, at this time, record my dissent to the statement of the universality of this accident.

The causes are numerous. It was formerly supposed to be by the pressure of the head; but latterly the opinion has been stated—which I cannot deny, but which, never having seen any reason to believe, I am inclined to think originated in the study, and not by the bedside—that it very frequently was caused by the sharp edge of the shoulder after the head had passed. The smaller dimension of the shoulders, in comparison with that of the vertex—especially when it is to be considered that one shoulder being delivered generally some little time before the other (thus diminishing the dimension of the part which would present at one time) renders rupture from that cause improbable, unless from some sudden expulsive act on the part of the patient or attendant.

The remote cause may be considered, in the majority of instances, to be the rigidity of the perineum; the proximate cause has generally been attributed to the want of proper attendance at the time when the head begins

* London Lancet; Report of London Med. Soc., Feb., 1852.

to press upon the soft parts. Support to these, by the hand of the accoucheur, would strike every one, either learned or ignorant, as a necessity, if he observed the appearances of the parts when the head threatens to pass through the slight and yielding barrier which opposes its exit. All instruction, whether from books or from teachers, inculcate support to the perineum at this period as all important; we cannot judge, therefore, how much actual benefit is derived from this act. Certain it is, that laceration and rupture do occur, occasionally, where this means is not neglected. Simpson says, "The proper management and support of the perineum no doubt modifies and diminishes this form of perineal lesion; but it fails far more frequently than is generally supposed in entirely preventing it."

Murphy says, "The young practitioner, fully impressed with the importance of preventing laceration, hardly ever commits the mistake of being too late in attending to this point. He very generally errs on the other side; he presses against the perineum a great deal too soon, and causes unnecessary heat and irritation in consequence, which rather retards its distention. * * * Again, when the head is nearly protruded through the vulva, anxiety to save the perineum may be the cause of its rupture. For instance, if you attempt to draw the perineum back over the head, it will be stretched too suddenly over the bi-parietal measurement, the widest part of the head. If, on the other hand, you push the head too much forwards, pressing, with the pains, from the sacrum towards the pubis, the same effect will be produced in a different manner; you force the parietal portion of the head too rapidly through the vulva."

A more common cause, as I conceive, is the sudden and extremely forcible contraction of the uterus, combined with the powerful efforts of the mother, bringing the head down strongly upon the perineum, undilated and unprepared for dilation, with such force as to tear it asunder. The only case that has occurred in my own practice, where rupture requiring surgical treatment has ever occurred, was of this character. At nearly the full time, with her first child, a woman struck her abdomen against the banister, which was followed immediately by a discharge of blood. On being sent for, at 10, A. M., I found the hæmorrhage ceased; the os undilated; no pains. Desiring the woman to remain quiet, I returned at noon: there was no change. At 3½, P. M., she had experienced no pain. At 4½ I was hastily summoned, and found that, shortly after my leaving, she had felt a desire to evacuate her bowels, and went up stairs to use the chamber. While upon it, with one pain child and placenta were suddenly thrown into it. Some days after, she complained of soreness, and I found a rupture extending through the sphincter ani.

The misapplied and improper use of ergot sometimes produces this result,

from the same cause as just stated. Dr. Crisp (of London)* had seen but one bad case of laceration, and in that ergot had been given. I cannot but think that the administration of ergot, often so prompt to act, and with great rigor, has been a frequent cause of this lesion.

The position of the mother, at the time of the birth, may sometimes cause this injury. One case I have seen, where this rupture was effected, was where the woman was walking about the room at the moment the child was born. The erect posture may be therefore supposed to be sometimes the cause.

The first duty of the accoucheur is to strive to prevent this accident; one of the most grave in its consequences that can occur as the result of labor. This is to be effected by close attention to the progress of the labor, so that when the head is in a situation where a sudden expulsive act would force it rapidly through the vulva, or the parietes adjoining, the hand be then so placed that by gentle pressure it may guide the advancing part through the proper channel; to caution the mother against adding to the strength of the involuntary muscular contraction by any expulsive efforts of her own; and to restrain any improper movements of the patient at this very critical and painful moment.

From what I have said before, some injurious results may follow the improper pressure upon the perineum. It is in vain to attempt to prevent the advance of the head by any pressure. If the force, therefore, be presented exactly in the axis of the advancing portion, advance may indeed be arrested in that direction, but only to turn it in another. If the pressure be made directly upon the perineum, spread out thin over the vertex, the head will take a new direction and pass through the perineum, or through the intestine, and ultimately through the sphincter ani. Instances of this I have known, when the attending physician stated that "he foresaw a laceration or rupture of the perineum, and made, therefore, very strong pressure upon the perineum to prevent it, but that this result occurred in spite of his efforts." It is not improbable that nature would have effected the delivery with less injury than a recto-vaginal fistula, if not without any.

When the pains are very vigorous, and the labor is almost instantaneously finished, the only practicable way for the accoucheur to act is by making this pressure. Ramsbotham, Rigby, Churchill, Murphy, and many others, advise each different methods to accomplish this result, whether with the right hand or the left; with the palm or the fingers; with the hand horizontal or transverse; with a napkin or without. These distinctions are to me apparently useless. Much depends upon the position of the woman, whether on her back, or right or left side—upon which side the attendant

* Med. Soc., of London. Report, in *Lancet* of February, 1852.

is placed—whether he has equal facility in the use of each hand, &c., &c. It is sufficient to say that a certain result is to be accomplished, and to leave it to the natural capability of the attendant to devise the most practicable method for obtaining this end. The point to be desired is to so make pressure upon the perineum, as to guide the advancing part through the natural passage; and the hand should be so placed, and the pressure so made, as to form an inclined plane with the hand from the anus to the vulva, the pressure being strongest at the anus and graduated down, so that the resistance shall be the least in the direction in which it is desirable for the head to pass. The most convenient and effectual way to do this is the best.

When, however, the pains are less rigorous, and the danger of laceration arises from the continued pressure upon a firm, muscular or cicatrized perineum, and when the delivery is protracted, there is then time for consideration and action. Interference with this state of things may here, with propriety, be admitted. And here it is where injudicious treatment sometimes produces what is most dreaded. The administration of ergot at this point of the labor is often of great utility; but it should not be given without deliberation, for it is, as has been before mentioned, the frequent cause of injury.

Blood-letting from the arm will often produce relaxation of tonic rigidity. Full doses of opium, so as to arrest the pains until relaxation may be produced by the natural processes, has been tried with advantage. Inunctions with warm oil, and fomentations with cloths wrung out hot from a decoction of poppy-heads, hops, or simply in hot water, may have a beneficial effect, especially in conjunction with other general treatment.

All attempts to dilate the parts by forcible traction, with the fingers upon the rigid perineum, are hazardous, especially during the pains, and are generally of little avail. The judicious application of instruments under certain circumstances, I need not mention, as that operation falls under a different head than that now under consideration.

By far the most effectual method that I have seen for cases of this character is the use of anæsthetics. To those that have used chloroform and sulph. ether, in labor, I need not enlarge upon the wonderful effects of these agents in not only mitigating the pains, and greatly hastening to a close a labor threatening to be a tedious one, but in relaxing the tonic contraction of the muscles of the perineum. Chloroform is doubtless of great assistance and utility in surgery, and for relieving pain in diseased systems; but in no branch of the healing-art are its divine virtues so apparent or so important as in the cases of difficult labor; and I should think it actually criminal in myself did I not, at every proper opportunity, bear witness to its miraculous efficacy, not only in relieving the pains heretofore incident to mortality, but in rendering operations (such as turning, &c.) heretofore

impossible, feasible and easy, and a means of saving ten times, aye, one hundred times, the number of infants' lives that would otherwise be sacrificed, to that of one mother who has died under the alleged effects of this potential remedy.

Chloroform, in dystocia from rigidity of the perineum, is a remedy of invaluable utility; it saves not only the mother from rupture and lacerations, but the child from death. The excessive expulsive pains of the mother are diminished; the parts are not only relaxed by the powers of this agent, but time is thereby given to them to soften and dilate from natural influences, so as to render the passage of the child with safety. The life of the offspring is preserved, by restraining the pressure to which it would have been subjected from the continued contraction of the uterus while awaiting the relaxation of the muscular structure. As a remedy or a preventive of rupture of the perineum, it consists in its double power of diminishing the force of the voluntary, and, to a degree, of the involuntary, expulsive efforts, and particularly of its marked effects, in relaxing the sthenic contraction of the parts.

Quite recently, in a recent English journal, I have noticed a proposition made by a physician of some note, to overcome the effects of rigidity of the perineum by a surgical operation; not by dividing the perineum, and thereby to exchange a torn wound for a cut one, but by dividing the labia majora—and minora also, if necessary—on each side of the vulva, about halfway between the pubis and the fourchette. The end proposed, is firstly to facilitate the delivery; secondly, to substitute a cut surface for a lacerated one; thirdly, to place this wound where the contraction of the parts would naturally bring the divided surfaces in juxtaposition, and at the same time to escape the lochial discharge, which is ever flowing, and which would prevent the speedy union of the lacerated or even cut edges of the divided perineum if the section were made through it.

This proposed operation, for I do not know that it has been tried as yet, is in its aim very excellent, but it is liable to some objections. In the first place, it is not certain that the operation, in the performance will effect the anticipated results. Like Sigault's division of the pubis, it may not add any thing to the size of the part requiring to be enlarged. The distention is needed below, and we do not know that dividing the labia majora will compensate for the want of distention of the perineum.

It is, next, a question, how much resort should be made to an operation of some physical pain to the mother—the cause, at any rate, of much mental anxiety—which make two certain wounds, instead of one which is problematical; which may be prevented by means already mentioned; which may be trivial; which (the chances are greatly in its favor) may never occur. For my part, I deprecate this operation entirely; and it is solely

from my belief in the inadvisability and impropriety of this operation, that I have written this paper reviewing the whole subject. Once propose and sanction this operation, now considered to be very rarely, if not almost never necessary; and in a few years you will be astonished at the increase of rigidities of the perineum, where the lateral section was absolutely necessary.

When the labor is impeded by a state of the perineum and labia dependent upon œdema of these parts, punctures may be made through the integuments, to evacuate the obstructing fluid, in such localities as may be deemed advisable; and this will sometimes save a partial or more general rupture in these regions.

But in spite of all our precautions, or in the absence of any proper attendant, this dire accident has occurred. In what manner shall we proceed to relieve it?

If the rent is partial, or if transversely to the perineum, nature will generally effect a cure. In almost any case where the sphincter ani is not divided, the duty of the surgeon is limited to the simply keeping the parts cleanly, subduing any local inflammation, and continuing the divided surfaces in coaptation. This latter duty is best performed by guarding the patient to the bed, upon the side; and, having placed compresses or small cushions between the knees and ankles, to pass a roller firmly from the ankles above the knees; and this should be allowed to remain until the adhesions are made permanent. The great difficulty in effecting this, arises from the lochial discharge, and very frequently adherence is not obtained until this is arrested. Not unfrequently, the parts are thus lacerated and no curative means are used; yet nature, after the lapse of some months, alone perfects the almost complete restoration of the parts to their natural appearance.

When, however, the rupture is through the sphincter ani, the case is really serious. Not only do excrements from the bowels constantly pass away, giving the unfortunate sufferer continual pain and annoyance from the loss of this support to the contents of the abdomen; causing prolapsus of the uterus, of the bowel itself, sometimes of the bladder; and preventing the slightest action; but it deprives the sufferer, in consequence of the disgusting character of the discharge, from mixing with society, and not unfrequently rendering life a burden. In no cases are the humane offices of the surgeon more needed or more useful.

If the laceration is recent, the proceeding is more simple. The parts are to be kept in juxta-position, and this is the difficulty. The first requisite which is absolutely necessary, the same as in operating for *fistula ani*, is to divide the sphincter, to thereby prevent the opposite surfaces from being drawn apart. From the peculiar formation of the sphincter, the

fibres commencing anteriorly and posteriorly, this object is best effected by making double incisions, one on each side, midway between the vulva and the os coccygis. The quilled suture should then be applied to the rent, taking the stitches nearly an inch back from the edges. After the stitches have been taken, and before tying, in order to save the obscenity caused by the flow of blood were this done earlier, the edges of the wound should be carefully pared, if the accident is not a recent one. The parts being thus accurately brought together, the bowels, which should have been previously thoroughly evacuated, should be kept quiet by opium administered in sufficient quantity; the bandage applied as before, the woman placed upon the side, and kept perfectly quiet. This operation may require to be partially repeated, especially if done while the lochia is profuse. If the rent in the sphincter is united, the cut opposite may be allowed to heal. Any ununited spots along the laceration may be advantageously touched with caustic, or again pared, and the parts brought together by the interrupted suture, if necessary. Great difficulty is found when the septum of the vagina and rectum is torn, and particularly when there is a loss of substance in this or any part. If the septum is ununited, the contents of the bowels are constantly evacuated through the vagina, and the unpleasantness of the matter is left still remaining. Mr. Brown, in a recent paper read to the London Medical Society, gives two cases of complete cure of this awful complaint by the above-described means.

In the single case treated by myself, and before mentioned, the interrupted suture, first applied, tore out in the course of a few days. The quilled suture, on the second application, on the sixth day after confinement, was entirely effectual, excepting a half-inch nearest the vulva, which after a few weeks united without further application. In this case, a few fibres of the sphincter remained, and the septum was not divided.

PART II.—REVIEWS AND BIBLIOGRAPHY.

Human Anatomy, Physiology, and Hygiene. By T. S. LAMBERT, M. D., &c. New York: Ivison and Phinney.

THIS is a book intended for popular use; that is, for teaching anatomy, physiology, and hygiene to the people, and not to the profession. Its merits are, therefore, to be judged by an entirely different standard from that to which we should submit a treatise intended for medical men, and professing to exhibit these sciences in their exact and accurate condition.

It has been our fortune at one time and another to be called upon to examine, and that critically, all of the treatises upon these subjects, which are intended for general use in our common schools and academies. Some of them, we feel free to say, have every appearance and bear every mark of having been written by men who at the best are but superficially acquainted with those sciences, and unfit in every way to be teachers in them. We have seen, too, the effects of the instruction conveyed by these books, and risk nothing in saying that so far from the knowledge imparted by them being beneficial either to the pupils or the public generally, the reverse is true. Popular physiology, as it is termed, is not under these circumstances a useful but an injurious branch of knowledge. Instead of anatomy and physiology made easy, they have presented physiology and anatomy filled with errors and blunders. This, to our own mind, has constituted a very serious objection to the introduction of this study into the common schools, or in any way as a subject of popular education.

But another difficulty, and this also a radical one, has presented itself to our mind. Starting from the proposition, which, by the way, is unsound, that the study of the structure of our own bodies ought to be one of the earliest studies, because we ought to understand ourselves before we proceed to investigate others; they have argued that it is a simple thing to become acquainted with the structure and functions of the human body. Thus a smattering of information on those points, has been mistaken for complete knowledge and understanding of them. Now, if the matter ended here, it would be harmless. But it has not. Charlatans, availing themselves of some of the catch-words of these sciences, and talking loudly of liver, stomach, spleen, kidneys, and so on, have flattered the people into a belief that by their knowledge of those subjects, they could appreciate the industry, the study, and the talent which have been required in their investigation into the virtues of some herb, or the proper compounding of some syrup, which, with its omnipotent antagonism to disease, has been so long unknown

to the whole race of physicians. The result has been not only the abandonment of medical advice to follow the instructions of the pretender, which is in itself a small thing, but as its result, a sad and unnecessary waste of health and life. For this very encouragement of quackery we have felt not only authorized, but compelled when in the position to require it, to give our opinion and our influence against this class of books.

To these objections, this book of Dr. Lambert's is not open ; at any rate, in this *revised* form we do not find that there is any fault in these respects. We have an impression, which we cannot now verify, that some of his earlier publications were open to our first objection. We are happy to say that to the second they have never been. A physician of education and intelligence, Dr. L. can appreciate the advantages and the necessity of the advice of physicians in sickness, instead of following popular whims, or the vagaries of men of one idea. For this course, we conceive that Dr. Lambert is not only to be commended for having followed the instructions of truth, but to be supported in his manly position. We have some reason to believe that it has not been without pecuniary injury that our author has continued to hold his ground ; and for that reason, if for no other, he should be supported by the medical profession.

This book is adapted to advanced classes in schools and academies ; and to those looking for a text-book for such students, we cordially commend it.

From the fact that it is to physicians that committees, superintendents, &c. look for advice as to the books to be used in these departments, we feel that it is proper and necessary that the profession should be informed upon this subject, and therefore make no apology for introducing a *school-book* to the profession by an extended notice. In conclusion, we may add, that it is to our mind no inconsiderable recommendation to the book, that it is well printed, on good paper ; and that the numerous illustrations are creditable to the publishers and the artists.

E. H. P.

A Practical Treatise on Inflammation of the Uterus, its Cervix, and Appendages, and on its Connection with Uterine Disease. By JAMES HENRY BENNETT, M. D., Member of the Royal College of Physicians, &c., &c. Fourth American, from the third and revised London edition. Blanchard & Lee, pp. 430.

ALTHOUGH the author has made some additions and alterations in the arrangement of this book, it still remains to all intents and purposes the same work that it was when first published in 1845. The positions at first taken by Dr. Bennett, being the result of a large experience both in France and in his own country, were too well established to require any great modification to be made in them. Their correctness, too, has been fairly

established by the success with which they have met the very decided opposition to which they have been exposed. It must be a source of great satisfaction to their author to know that the principles, in uterine pathology, first developed by him, and the modes of treatment first pointed out by him as those based on a rational therapeutics, are now acknowledged and followed by the most eminent and most successful practitioners of this department of medical science.

This work has been so long before the profession, and its principles are so fully known to the profession, that we do not feel called upon to enter into an extended analysis of its contents, or review of its doctrine and principles.

We regret to say that in reading this edition we have felt that the publishers have not fulfilled their task quite as they should have done. It is true that the typographical errors which are noticeable do not prevent one from understanding the author; but they are always annoying, and can, by a little more care, be entirely avoided.

E. H. P.

Elementary Chemistry, Theoretical and Practical. By GEORGE FOWNES, F. R. S., &c, Edited, with additions, by Robert Bridges, M. D., &c. Blanchard & Lee. 1853. Pp. 555.

FOWNES' Chemistry has been so extensively in use as a text-book, that there is no need of a minute account of its contents or arrangement. This edition appears to be taken from the fourth London edition, which was edited by Dr. H. Bence Jones and A. W. Hoffman. Dr. Bridges' additions bring the different departments down to the present condition of the science, and thus fit the book to continue to be a favorite book with those who are instructing students in this science.

E. H. P.

PART III.—CHRONICLE OF MEDICAL PROGRESS.

PHYSIOLOGY AND GENERAL PATHOLOGY.

On the Influence of the Nerves upon the Vessels of the Tongue. By Dr. M. SCHIFF.

As the inferior surface of the tongue of living dogs well exhibits the various degrees of injection of the small vessels, and since, in experiments upon one side, we have before our eyes the normal condition of the other for comparison; the author also endeavored to ascertain whether there are nerves the paralysis of which effects a dilatation of the small vessels. He

made all his experiments upon large dogs, and limited himself to the anterior two-thirds of the inferior surface of the tongue. He always satisfied himself, in the first place, that both halves of the tongue were normal and of equal redness.

In dogs in which the hypoglossus had been severed for weeks or months, the paralyzed half of the tongue was no more injected; on the contrary, in many the paralyzed side was paler. But if the lingualis also of the same side were severed, in ten minutes, or even sooner, the paralyzed half was perceptibly redder than the other, and remained so. But since, after division of the lingualis, the insensible half of the tongue was crushed by the teeth, and consequently very much torn after some days, this reddening might be considered as the result of a mechanical lesion. But that this was not the case, the following circumstances prove: The reddening lasted weeks and months, whilst the mechanical injury was manifest only in the course of the first three weeks. After division of the lingualis, the tongue gradually diminished; and when this had reached a certain degree, it was no longer wounded by the teeth, and its torn borders cicatrized, even if the lingualis were removed in its entire free course. The redness dependent on injury of the lingual border always occurred immediately next to the border, whilst the neuro-paralytic reddening was diffused at a later period over the whole half of the tongue, even to the median line; and, at first, this reddening was seen still more intense along the border. When the author excised the lingualis in animals otherwise uninjured, the color of the tongue beyond the inflamed border remained perfectly normal, except that occasionally lenticular reddish spots appeared. But if he also divided, in these animals, the hypoglossus, the paralyzed side suddenly reddened, even to the median line, and the reddening did not disappear again, if the excised piece of nerve was sufficiently large to prevent a reunion. The same result occurred when the hypoglossus was severed, several weeks after the lesion of the lingualis, and after cicatrization of the borders of the tongue. From these experiments, the author concludes *that the tone of the small lingual vessels depends as well upon the hypoglossus as upon the lingualis*, in such a manner, that neither of these nerves has special control of the limits of its distribution, but that all the smaller vessels in the parts observed, are equally under the influence of both nerves, so that the one can perform vicariously the function of the other, and also that the extent of diffusion of the vaso-motory power of the two nerves *is identical throughout*.

Relying on the results of former experiments concerning the *regeneration of the nerves*, namely, that in a bisected bundle of nerves, the vaso-motory power is very quickly restored, afterwards the sensitive, and still later the motory, our author removed a large section from the lingualis of seven

young dogs, and merely severed the hypoglossus. As before, so now, a general reddening of the injured half occurred. In five dogs, the paralyzed half of the tongue became gradually paler, from the 6th to the 16th day, and both sides equally colored, although neither sensation nor voluntary motion had returned; the rhythmic oscillation of the muscular bundles, which was observed by the author in former experiments, took place here. In two dogs, killed in the third week, the two cut ends of the nerves were connected, but galvanic irritation of the central piece had no effect. In one dog, however, killed at the end of the fourth week, the galvanic irritation had a slight effect, but there was no voluntary motion of this half of the tongue. This first appeared in one dog in the fifth week; in another, still later. In two dogs, which, after four weeks, still exhibited the redness of the inferior portion of the tongue, there was no regeneration of the hypoglossus. In three dogs, the author took a piece out of the hypoglossus, and merely severed the lingualis. The reddening of that half of the tongue disappeared again from the fifth to the eighth day. Slight sensation manifested itself, first in one on the thirteenth day. In another the author, on the eleventh day laid bare the lingualis; the cut ends were united, the peripheral portion yet destitute of sensation. As the nerve was severed anew, the reddening returned again. In the third dog, in which sensation was again perceptible after nineteen days, our author divided the nerve in the fourth week, for the second time, and took out a large piece: the reddening immediately recurred, and did not again disappear.

If these experiments prove that the vascular paralysis had disappeared, that of motion and sensation still remaining, while either nerve was in the process of regeneration, the question also arises, in what manner one nerve can assume, in some measure, the functions of another. The action of the two vagi in the production of stagnation of blood in the lungs is similar, but with this difference, that in the tongue a motor and a sensitive nerve enter into reciprocal action. Each vagus gives off fibres to many ganglia distributed in the lungs; each ganglion receives fibres from both vagi nerves; but these ganglionic nervous fibres, and not the original vagus fibres, supply the vessels of the lungs. Now, as each cerebro-spinal vagus fibre entering into the ganglion diffuses its excitation to an indefinite number of ganglionic globules, so that the latter may simultaneously rest under the influence of several nervous fibrils entering into the ganglion,—even so in all organs which possess ganglia, or whose nerves traverse ganglia, will the division or paralysis of the nerves of one side not have a result which corresponds at all to that of bilateral paralysis. This hypothesis farther explains, why, in hemiplegiæ, the activity of the intestines remains undisturbed, why the division of the vagus of one side does not disturb the action of the heart, &c.

If this hypothesis is correct, we should find, in the anterior part of the

tongue, ganglia which receive radical fibres, both from the lingualis and the hypoglossus, and the nerves originating from these ganglia presiding over the vessels of the tongue. The author prepared, with this view, the finest ramifications of the lingualis and hypoglossus in man, in ruminants, dogs, cats, &c., and discovered even in the finest twigs, especially of the lingualis, even to the point of the tongue, small microscopic ganglia in very great number, in which unilateral fibres of origin and many (6, 9, even 11) emergent nervous bundles, with smaller primitive fibres, were visible. Several times, also, he succeeded in isolating ganglionic spherules with emergent nervous fibres, which very soon, almost immediately after their exit from the spherule, underwent repeated divisions. When the author divided the lingualis and hypoglossus at the same time, he found, after a certain period, all the nerves in the anterior two-thirds part of the tongue of dogs degenerated, as well those going to the ganglia as the numerous nervous bundles, consisting of small primitive fibres, which proceed from them. When the author had excised a large piece of the lingualis, most of the filaments connected with the ganglia were altered in such a manner, that, where the incident branches were to be distinguished from the emergent, in the former frequently only a few unaltered primitive fibres were to be seen, whilst the latter in addition to a surplus of altered filaments always contained a considerable number of those which were normal. But, in the smaller ganglia, which were farther removed from the trunks, and whose radical fibres were themselves derived from ganglia, all the branches exhibited very many unchanged primitive fibres. The ends of the original fibres of the lingualis, which were distributed to the mucous membrane, were completely atrophied. After division of the hypoglossus (the lingualis intact) our author could distinguish in the radical trunks of the ganglia, single broader and altered primitive fibres: among the emergent he saw only normal fibres. If the latter does not prove that *all* the emergent fibres were normal, it nevertheless follows, that in this case far less fibres were altered than had become so by division of the lingualis.

In order, now, to decide whether the nervous filaments which go from the lingualis and hypoglossus to the ganglia of the tongue, are *homogenous*, vasomotory nerves, or whether it is sufficient that nervous filaments in general (motory or sensitive) pass to the ganglia in order to receive their power, the author instituted the following experiment: In eleven dogs, he excised the central end of the lingualis as high as possible, and removed the periphtric end of the hypoglossus up to its distributions in the tongue; the cut ends he united with a slender silk thread, carried through the outer envelope of the nerves, and only slightly drawn. In the paralyzed half of the tongue the usual reddening occurred, but in five dogs diminished, from the fifth to the ninth day, and a few days later had entirely disappeared; in the other

six, however, it persisted continuously. Three of the five first, in which the half of the tongue exhibited neither sensation nor motion, were killed in the third week. The two cut ends were united by a greater or smaller node, partially fused with the neighboring parts; only in one instance were new nerve-fibres clearly to be seen. The peripheric extension of the lingualis exhibited everywhere, besides a preponderating number of altered fibres, yet many normal ones, partly of larger, partly of smaller diameter. In the nerves which were connected with the ganglia, the unaltered fibres preponderated over the broader nerves of the mucous membrane. The proper nerves of sensation were, like those in the interior of the muscles, all atrophic. Here a partial regeneration of the vasomotory nerves, and through the latter a union of the lingualis and hypoglossus, must have taken place. The two other dogs, in which neither sensation nor motion had returned, were killed in the eighth and ninth week. Here were the two stumps united in one uniform, continuous trunk, in which the point of division could no longer be recognised. The microscopic examination of one preparation gave results analogous to those just mentioned. This shows, *that the motory and sensitive fibres cannot unite, but that the vasomotory fibres arising in common from the lingualis and hypoglossus become regenerated.* In the six other dogs, no union had taken place.

As regards the condition of the vessels after division of the nerves of the tongue, those of the inferior surface and of the muscles of this organ acted in a manner analogous to those of other voluntary muscles, whose nerves had been divided. The author, therefore, presents his experiments upon this point in general. In order to apply pressure in the most equal manner possible upon the morbid and healthy side, he always injected both sides at once from a common trunk; the tongue from the ascending aorta, the inferior extremities from the abdominal aorta. After long-existing paralysis, in consequence of division of the nerves, the author found frequently, but not constantly, both the arteries of the thigh, the shoulder, and those of the tongue, together with their most important ramifications upon the paralyzed side, of somewhat smaller diameter than upon the healthy side. But this condition is probably only an indirect result of the nervous paralysis. For example, the circulation is impeded by the contraction of the muscles, and by the pressure resulting therefrom upon the smaller vessels; and since the lateral pressure of the blood in the larger vessels is thereby exalted, the latter must be distended; but if the muscles are paralyzed, the larger arteries appear smaller than upon the sound side. This view becomes the more probable, since in two dogs, six months after exsection of the hypoglossus without lesion of the lingualis, the author also found the arteries of the tongue smaller upon the paralyzed side.

If we examine a thin section of an injected muscle of the paralyzed side,

together with a similar section of the same muscle of the sound side, under a magnifying power of about twenty-five times, we see the larger vessels of both sides nearly alike, but in the piece from the paralyzed muscle many more small vessels of about $\frac{1}{30}$ to $\frac{1}{40}$ of a millimetre in diameter. We see indeed, upon the sound side small vessels of the same diameter, but only in particular parts, and these very soon diminish in size, since they ramify farther. Upon the paralyzed side, on the other hand, the vessels which have once reached this diameter, continue the same for some distance in their farther course almost unaltered; they do not diminish gradually, but pass almost immediately to vessels so narrow that they are imperceptible to a moderate degree of magnifying power. In like manner the small veins do not gradually increase, but pass suddenly to a greater diameter. The observation of the true capillaries is more difficult upon the paralyzed side, because they are covered by fat vesicles; here, also, the fine capillary meshes pass suddenly to broader vessels. *In the paralyzed parts, the smaller vessels which have contractile walls, are also perceptibly dilated.* (Waller.) The paralyzed side appeared always richer in blood-vessels, because only the dilated ones were visible, the finer of the sound side being withdrawn from the eye. In the skin, the same relations take place; so also in the periosteum and cellular tissue. This dilatation of the smaller vessels may be seen in the eye without injection, after division of the trigeminus, and in the ear of dogs after division of the nerves of the auricular muscles.

Notwithstanding the dilatation of the small arteries and veins, upon which the red color of parts rich in blood depends, the muscles of paralyzed parts appear no redder, but paler than normal. It follows, therefore, that the redness does not depend alone upon the vessels, but is also essentially inherent to the muscular tissue. If the latter is less nourished, it becomes also more colorless and transparent. Thence comes, too, the deposition of fat in the muscles of long paralyzed parts. For example, many fatty particles in part appear in the enveloping membrane of the bundles along the course of the vessels, in part certain muscular fibres themselves undergo a fatty metamorphosis. In consequence, also, of this infiltration of fat, long paralyzed muscular parts and especially the tongue become, after death, very quickly discolored, greenish, and soft. Fatty degeneration of the muscles is in part the result of their inactivity; for we frequently see it, though in small degree, after mere division of the hypoglossi, without lesion of the linguales. The cellular tissue of paralyzed muscular parts always consists of a greater number of layers, than in the healthy parts.—*Schmidt's Jahrbücher, January, 1854.*

On Diseases of the Heart. By Dr. TUPPERT, of Erlangen.

The author bases his treatise upon a case of pericarditis observed at the clinique of Prof. Dittrich, of Erlangen, and enumerates in conclusion those diseased conditions which produce a more or less *diminished contractile power of the heart*. To these belong:—

1. *Pericarditis*. Rokitsky affirms that the influence of pericarditis consists in a paralysing of the muscular substance of the heart, which immediately leads to passive dilatation; that this effect is so much the greater the more the pericarditis has been of a chronic character, the exudation purulent, hæmorrhagic, or tuberculous; and that the dilatation especially becomes the more permanent, the more the congelations have formed themselves into a thick, dense, unyielding tissue enveloping the heart. The author adds, that it is not alone in the forms of pericarditis just mentioned, that heart-paralysis is threatened, but that also acute, even benignant, simple, sero-fibrinous exudations in the pericardium may induce the same condition. The bond of union between exudation in the pericardium and cardiac paralysis does not consist merely in the pressure which the exudation exercises upon the heart, thereby obstructing its motions, neither in a sort of imbibition of the surrounding exudation by the substance of the heart, but is based upon a well-known anatomico-pathological law, that in the more important exudative processes in any tissue whatever, and especially the mucous and serous membranes, the contiguous textures take part in a more or less important degree, in the form of acute oedema, or acute serous infiltration. Experience teaches that the quantity of the pericardial exudation is not the only determining cause, but that even in small exudations, especially purulent, the cardiac muscle presents the symptoms of paralysis; although in this latter case the whole blood-mass, and that circulating in the vessels of the heart itself, and nourishing and exciting the heart in an anomalous manner, may have their influence.

2. To pericarditis are joined *Myocarditis* and Endocarditis, diffused over a large surface. Here also diminished contractility of the heart is induced by participation of those parts of the substance of the heart contiguous to the inflamed points, in the form of hyperæmia and serous infiltration, and the swelling and sponginess induced thereby.

3. An important cause, though rarely occurring, consists in *sudden, immoderate distention of the heart with blood*. This cause never affects the whole heart in all its cavities simultaneously; it is, nevertheless, especially important when it occurs in the left ventricle. As an example may be mentioned the sudden repletion determined by insufficiency of the aortal valves, arising from acute causes, and immoderate distention of the left ventricle with blood; death follows in consequence of paralysis of the heart.

Far more frequent than this sudden, immoderate distention of the heart, is a cardiac paralysis, when this organ is suddenly or gradually put in the condition of fatigue in consequence of previous severe efforts, especially from the existence of mechanical hindrances. This happens particularly in hypertrophied hearts.

4. In the course of acute affections, as typhus, acute exanthemata, &c., already during life a remarkable relaxation of the muscular substance of the heart occurs, with its well-known symptoms, as if the expression of an affection continually becoming more severe. The cause of this symptom is here, either a local one, that is, an anomalous nutrition of the heart by anomalous blood; or, what is more probable, a central one determined by an anomalous nutrition of the brain and its nerves, and anomalous innervation of the heart thereby induced. In the corpse this symptom has its counterpart in the so-called passive dilatation, collapse, lacerability, decoloration, saturation with red blood. To this diminished innervation of the substance of the heart dependant on the central apparatus of the nervous system, are joined on account of similarity of cause, all those cases of acute and chronic diseases, belonging to the class of brain diseases, in which, through whatever cause, the innervation becomes altered.

5. An important diminution of consistence of the cardiac substance accompanies fatty disease of the heart, whether it be that this depends upon an accumulation of an unusual quantity of fat upon the surface of the heart, or upon fatty degeneration of the cardiac muscles. * In all these fatty diseases a disproportion of tissue to the powers of innervation takes place, and, consequently, diminished contractile power.

6. The atrophic conditions of the heart, by whatever determined, are always accompanied by diminished contractility of the heart-muscle.

7. *A priori*, another cause may be conceived, to which, hitherto, little reflection has been given, namely, the saturation and sponginess of the heart-substance from imbibition in hydrops pericardii.

8. If we comprehend under the name diminished contractility of the heart, also those conditions in which the texture of the heart presents no perceptible alterations, then is there a condition to be considered, which indeed occurs symptomatically, but may also occur absolutely. It is neurosis of the heart (neuralgia plexus cardiaci), which is announced, in many cases at least, not by immoderate palpitations of the heart and excitation of the vascular system, but by functional depression of its central point. The observation of such a disease, with its subjective sensation of sinking, with the feeling that the heart is pressed together, held fast, that it ceases to beat, with the perception of disturbed respiration, of symptoms of pressure upon the heart, of small pulse, of slight or severe swooning, &c., teaches that we have to do with diminished contractile power of the central organ of circulation.—*Schmidt's Jahrbücher*, Jan., 1854.

OBSTETRICS, AND DISEASES OF WOMEN AND CHILDREN.

Herpes of the Vulva. By F. J. LEGENDRE, D. M., Physician to the Hospital of Louraine.

Etiology.—The causes of the development of herpes about the vulva, are both predisposing and exciting. These two orders of causes may act separately, but most commonly they are united in the same person.

Among predisposing causes, I will mention obesity, warm weather, the natural acrimony of the secretions from the vulva in some women, the menstrual periods, and pregnancy.

In very fat women, the genito-crural folds, in consequence of the protuberance and contact of the upper and inner part of the thighs, present a very marked depression, or cavity, in which the labia majora are closely packed. These parts, in which active secretion is constantly taking place, being thus in intimate contact, it is readily seen that this arrangement will favor the development of herpes of the vulva.

There are some women, especially among those of a reddish or very dark complexion, whose sweat and whose mucous and sebaceous secretions are very abundant, very acrid, and very offensive. This state of things, which is habitually produced in some women through an organic disposition, is often caused in many others by hot weather; thus, these two causes, both isolated and united, may be considered as favoring the development of herpes.

The approach of the menstrual period is also a predisposing cause of herpes of the vulva: at this time the vulva participates in the congestion of the pelvic organs, furnishes a more active and abundant secretion from its glandulæ, and is often the seat of pruritus; circumstances which are fully adequate to explain the tendency of herpes to appear at this time. Thus it is that some women, a day or two before each menstrual period, are affected with a herpetic eruption, consisting of one or more groups of vesicles, which either dry up or give rise to superficial erosions. These erosions persist as long as the periodical flow continues, and finally cicatrize without leaving any scar, a few days after its cessation.

Finally, pregnancy, by interfering with the capillary circulation of the abdominal organs, as shown by the violet and sometimes almost black color of the mucous membrane of the vulva and vagina, and by exciting in most cases an abundant creamy or muco-purulent discharge, is equally a predisposing cause of herpes. Indeed, the abundance of the discharge which bathes the vulva, added to the stagnation of the blood, excites pruritus, the effect of which is to favor the development of herpes.

Exciting Causes.—Discharges from the vagina are a frequent exciting cause of herpes of the vulva; but this cause acts only in those cases where the discharge, constantly bathing the vulva, irritates it by its abundance and purulent nature, and excites pruritus which the patients endeavor to allay by scratching themselves with their nails, or rubbing themselves with their chemises, which are often of coarse material, and always soiled and stiffened by the dried matter of the discharge. A vesicular eruption appears almost immediately after such repeated irritations, and, when questioned on the origin of their symptoms, women reply, that they first noticed an abundant and greenish discharge, followed by itching of the parts, and, after scratching themselves, small pimples or painful erosions of the vulva appeared.

Another exciting cause of herpes of the vulva is want of cleanliness, which acts in nearly the same manner as purulent discharges from the vagina. From this cause, the sweat, and the secretions from the mucous membrane and sebaceous follicles, which are so active in this region, are retained in contact with the part, and, undergoing decomposition, irritate the vulva, and excite pruritus, which is soon followed by a vesicular eruption.

Frequent and long walks often excite herpes of the vulva, from the friction between the latter and the upper and inner part of the thighs, and from an increase of the secretions which lubricate the parts, thus exciting irritation, which is well calculated to give rise to this eruption. It is true, however, that this cause acts especially on fat persons, and more particularly in hot weather.

The last exciting causes which I will mention are, in adults excessive coitus, and in young girls masturbation and attempts at rape; the latter consisting, in most cases, of mere friction of the surface of the vulva with the virile organ. But it is important to be noticed that, in these cases, the cause is often complex, from the coëxistence of irritation from a vulvo-vaginal discharge, which either depends simply on the amount of violence exercised, or has been communicated by the guilty party; but, whatever its nature, the contact of acrid and irritating matter with the delicate epidermis of these parts, acts as an exciting cause of herpes, in addition to the act of violence.

General symptoms of herpes of the vulva.—Herpes of the vulva appears under two different aspects, according as the eruption consists of one or two clusters of vesicles, or a large number of vesicles scattered or grouped together. In the first case, the labia majora are neither red nor tumefied, nor the inguinal ganglia increased in volume; the patient experiences scarcely any pain, and only a slight smarting sensation in the part. The

disease is rarely observed at its outset, when it is characterized by a group of five or six globular vesicles, of the size of a millet seed, filled with a perfectly transparent, serous fluid, of a citrine color, and surrounded with a rosy areola of greater or less extent; generally, when first observed, the vesicles have become flattened, wrinkled, and withered, and the serous fluid within, lactescent; or they may have become enlarged and have run together, tending to form a bulla beneath the epidermis. Again, not unfrequently, when the patients are examined, the vesicles or bullæ have disappeared; and in that case the affection is recognised by a very superficial erosion, which is rounded, grayish, surrounded with a rosy areola, and situated either on the external or internal surface of the labia majora. This erosion, resulting from the removal of the epidermis from the vesicles, and from the narrow interval which separates them, might deceive a person who was not on his guard, and be taken for a chancre, especially when situated near the entrance of the vagina, and, above all, at the fourchette.

When herpes of the vulva occupies numerous points on the external and internal surface of the labia majora, and even, as often occurs, on the perineum and margin of the anus, the patients complain of a burning, smarting sensation, and severe pain in the parts. This pain is exasperated by the contact of the urine in micturition, and also by walking, which is sometimes almost impossible, it is so painful. On examining the parts, the labia majora are found to be more or less tumefied and reddened, and also the margin of the anus, if it participate in the disease; in the next place, our attention is immediately drawn to the presence of numerous ulcerations on the external or internal surface of the labia majora, or, more particularly on their free border. Sometimes these ulcerations are confined to the vulva; and sometimes the margin of the anus, the perineum, and even the upper and inner surface of the thighs, corresponding to the free borders of the labia, present similar ones. These ulcerations may be covered with small brownish scabs, or their surface be free, and discharge a sero-purulent or purulent fluid; in both cases they present the following characteristics: they are generally superficial, constituting erosions rather than true ulcerations; they are regularly rounded, their edges clearly cut, their floor grayish; their size that of a very small lentil when they are isolated, but larger and more irregular when they result from the union of several neighboring ulcerations. Though these ulcerations are generally very superficial, and involve only the deep layers of the epidermis, some of their number simulate true chancres by their more elevated and perpendicularly cut edges, and their grayish floor. Finally, the illusion is heightened by the development of small rounded ulcerations, with a grayish floor, on those points of the internal surface of the thighs which correspond to the ulcerations on the free border of the labia. These ulcerations appear to be produced by the inoculation of a

virulent fluid furnished by the ulcerations on the labia, whilst they are, in fact, only the effect of the presence of a simple irritating liquid upon two corresponding surfaces in contact.

The practitioner, in pursuing his investigation, rarely fails to find other lesions which indicate the true nature of these ulcerations, viz.; globular vesicles, either isolated or more frequently in clusters of five or six, of the size of a millet seed, distended with a transparent serosity of a citrine color, and often surrounded with a narrow, rosy areola. When these vesicles are a day or two old, their characters change; their fluid becomes lactescent; the epidermis is wrinkled, withered, and finally torn; then the superficial layers of the cutis are exposed; and thus the original herpetic vesicle, by undergoing successive modifications, or a kind of gradual degeneration, is transformed into an ulcer of greater or less depth. Sometimes instead of the separate vesicles breaking, they first extend over a wider surface and unite, giving rise to an elevation of the epidermis resembling a bulla, which finally bursts and sometimes becomes covered with a pseudo-membrane. At other times, the fluid included within the vesicles, after becoming seropurulent, becomes concrete, forming a brownish scab of the size of a hemp seed, and covering a small ulceration. The clusters of herpes are easily discovered, when they are situated on the internal surface of the labia, at the margin of the anus, or on the perineum; but if they be not found at first, before denying their existence, we should carefully examine the external surface of the labia majora, stretching the skin so as to efface the wrinkles and folds, in the midst of which the vesicles are often concealed. In the cases which have come under my observation, I have almost always found some of the characteristic vesicles among the numerous ulcerations scattered over the vulva; but it is evident that this feature may finally disappear, when the ulcerations are kept up beyond their usual duration by negligence, want of cleanliness, or acrid secretions. Still, these cases are rare, for the causes which keep up the ulcerations continue to generate new vesicles also.

Whenever the vulva presents several herpetic clusters, the lymphatic ganglia of the internal portion of the groin are always tumefied and slightly sensitive on pressure, the tumefaction and sensibility being proportioned to the extent of the eruption and the number and depth of the ulcerations. This engorgement sometimes acquires the volume of a small pigeon's egg, but it projects but little from the surface, and its size is better appreciated by the touch than by the eye; I have never seen it accompanied by redness of the skin, nor terminate in suppuration; but its entire resolution is quite slow, often not taking place till some days after the ulcerations are already cicatrized.

Prognosis.—Herpes of the vulva, in itself considered, is a very innocent disease, and its prognosis possesses so little gravity, that it would scarcely deserve special consideration, if it were not that its situation, and the ulcerous form which it affects, may become the source of error, very prejudicial to the patient, and even to the reputation of the physician. Considering the prognosis of herpes of the vulva, therefore, not absolutely, but relatively to the circumstances in which it is developed, we see that it rests almost exclusively on the time that it takes to heal. Thus, when this eruption and its ulcerations appear on the approach of, or during the menses, after venereal excesses, after too great exercise on foot, or from want of cleanliness—since these causes are temporary or easily remedied—the herpes disappears rapidly, and its prognosis will be favorable; on the contrary, it will be less so when it is developed under the influence of a constitutional diathesis, or a purulent discharge from the vagina—a disease which is always slow in disappearing; and in this case frequent relapses will occur, or the eruption will be kept up for a long time, either because many successive crops of vesicles are developed, or because the ulcerations succeeding the primary group of vesicles are slow in cicatrizing, or even increase in depth, soiled as they are by the purulent matter which flows from the vagina.

Treatment.—When herpes of the vulva consists of one or two groups of vesicles, it is an affection of little importance, and the most simple means are sufficient to dissipate it. The patient should be directed not to walk, to resist the severe pruritus which she experiences, and to apply frequent lotions of a cold decoction of nightshade, unless the menses be present, when the lotions should be warm.

But when, from the patients' indulging in walking, or allaying the itching by scratching themselves, the vesicles have assumed the form of painful ulcerations, various sedative remedies should be employed; such as baths prolonged for an hour in a warm decoction of bran; the constant application of cataplasms of potato starch and marsh-mallow water, renewed every five or six hours and applied directly to the vulva, each application being preceded by a lotion of a decoction of nightshade, to remove any remains of the preceding poultice which may have become sour. The horizontal posture and diluent drinks, joined to a mild and spare diet, make up the constitutional treatment. Injections of warm water, or of a decoction of leaves of nightshade, should be used when herpes occurs during the course of vaginitis; but if during the catamenial period, the treatment should be confined to keeping the horizontal posture, and the frequent use of warm lotions. Under the influence of these means, the severe pain which sometimes attends the herpetic ulcerations, rapidly subsides; and a similar effect is produced on the grayish aspect of the ulcerations, which often disappears

within twenty-four hours, giving place to a rosy color of the surface, and the ulcerations themselves cicatrize completely by the fifth or sixth day. If among the ulcerations which are sometimes developed upon the vulva in considerable numbers, there be some that retain their sensibility, and refuse to cicatrize in spite of the use of these emollients, either because their situation (as in the neighborhood of the vagina, at the margin of the anus, or on the perineum) exposes them constantly to the contact of irritating fluids, or to repeated friction, it is well to substitute for the emollients slight cauterizations with the solid nitrate of silver, which destroys the sensibility of the ulcerated surfaces and favors their cicatrization. Finally, if, after cicatrization, a tendency to hypertrophy show itself in the ulcers, we may anticipate their spontaneous disappearance, and hasten their resolution, by touching them repeatedly with nitrate of silver, or, what is also very good, with oil of cade.—*Archives Générales de Médecine.*

SURGERY AND SURGICAL PATHOLOGY.

Observations on the Human Eye by means of the Speculum Oculi.

[The first and second numbers of the *Deutsche Klinik* (Berlin) for January, 1854, contain an able article by Dr. Oscar Sæmann, on the *Speculum Oculi* of Helmholtz, and its application to the diagnosis of diseases of the organ of vision. The first part of this paper is chiefly devoted to a description of the mechanism of the instrument, the manner of its application, &c. The second part is that which we present to the readers of the MONTHLY, under the above title. H. N. B.]

In order that we may be able to distinguish with our eye the pathological alterations existing in any organ, it is necessary that we should be familiar with the appearances which that organ presents in its normal condition. It becomes necessary, therefore, that I here give a brief sketch of what we perceive in the healthy eye by means of the *speculum oculi*.

If we give the reflector such a position that it sends the rays of the taper passing through the convex glass into the eye to be examined—by which the dark spot, which corresponds to that point of the speculum not covered with foil, must fall directly upon the pupil—and look through the transparent portion of the reflector, we see the pupil clearly illuminated. The degree of its lucidity is, under otherwise equal circumstances and with a sufficiently dilated pupil, different in different eyes, and depends upon the greater or less capacity of the background of the eye to transmit and reflect light. A part of the rays of light, for example, is reflected by the retina

and its vessels; another part passes through these to the choroidea. Of this transmitted light, again, one portion is reflected by the vessels of the choroidea, whilst another is absorbed by the pigment of this membrane, and the remaining portion penetrates the more transparent parts of the same, to be at last reflected by the sclerotica. The stronger the pigment of the choroidea is developed, the more the light is absorbed; the less it can penetrate, so much the more dimly will appear the illumination of the pupil: the less the pigment is developed, the more the light is reflected; so much the clearer will be the pupil. The color of the light presents all shades, from whitish-yellow to yellowish, from yellowish-red to the finest rose, which latter color I observed in an albino. Van Trigt directs attention to the fact, that the abundance of the choroid pigment is in direct ratio with that of other tissues, especially the hair; and that consequently in blonde individuals the pupil appears much clearer than in individuals with brown or dark hair. If we now apply to the instrument the concave lens corresponding to the state of refraction of both eyes, we perceive, in the background of the eye, vessels of larger or smaller size, which sometimes run isolatedly, sometimes so that artery and vein lie together. The background of the eye itself appears in a reddish light, which passes to a dark brown when the pigment is very abundant, but shines of a clear rose color when the choroid pigment is less. The particular parts of the retina also do not appear equally colored; the color is clearest round about the *optic nerve* and grows gradually darker towards each side. If we direct the eye somewhat inwards, it is not difficult, after some practice, to discover the *Papilla nervi optici*, which exhibits a truly splendid appearance. It presents itself generally as a circular, more rarely as an elliptical, clear white disk, which, shining like the full moon in a blood-red sky, is more or less netly bounded by the surrounding parts, and at its periphery is girdled by a dark, often not wholly closed ring of various breadth. At times, here and there, single dark spots may be distinguished upon the shining disk, caused by little inequalities of the *papilla*. Somewhat inward from its middle point issue the *Arteria* and *Vena centralis*, the first of which is marked by its clearer red color and smaller circumference, sometimes single, sometimes forming a coil. For the most part these vessels extend upwards and downwards over the *papilla*, and divide near its periphery into two or more branches; but they do not always take so regular a course, but wind about in all directions, sending out their twigs over this disk. The artery, as well as the principal trunk of the vein, exhibit at the summit of their curve a light streak, which proceeds from the reflected light, and is not observed in the neighboring venous branches, because their walls are too little arched, more level. The falciform line of shade lying inwards from the *papilla*, which, Helmholtz has

always, and Van Trigt never, seen, I have also at times, but not always, been able to discern.

If we cause the eye of the individual examined to look directly at the image of the taper present in the reflector, we have before us the point of direct vision, the *macula lutea*. Helmholtz says it rises less abruptly from the surrounding parts, has a darker gray-yellow color, and shows no vessels. According to our observations, however, it is distinguished in nothing from the rest of the background of the eye, neither by a different color or a greater want of vessels.

If we examine the normal eye with convex glasses, through which the eye of the observer is adapted also for the anterior parts of the same, *cornea*, *iris*, *lens*, *corpus vitreum*, we see the magnified pupil shine with uniform clearness.

After this short description of that which we perceive by means of our instrument in the normal eye, I proceed to the observations which we have made upon the diseased eye.

Diseases of the Lens.—The most frequent diseases of the lens are opacities, *cataractæ*. When the opacity is far advanced, and its color a clear gray, there is no difficulty in the diagnosis; but the cognizance of opacities in their first commencement is extremely difficult, and the difficulty is increased from the fact that in elderly persons generally the pupil does not appear of a pure black. But, by the aid of the speculum it is possible to detect even the slightest opacities of the lens and its capsule, since the latter are very accurately defined when the background of the eye is illuminated, and we examine through a convex lens of 3—6" focal distance.

We have observed commencing opacities of the lens, which were wholly inaccessible to observation according to the ordinary methods of examination, in 24 individuals,—eight of whom had a perfectly normal vision, and were examined only *experimenti causâ*, the rest suffering from disturbed or extinguished vision, which had its origin, however, in other diseases of the eye.

The most frequent opacity was that of a nucleiform cataract (*Kernstaar*), and was absent in only three of these 24 individuals. Upon application of a convex lens, No. 3, it was seen in all gradations of size, from the smallest point to a deep black disk of nearly 1''' in diameter, and was always conformable, no single dark atoms being distinguished as composing the opacity. Its periphery was mostly circular, in two cases elliptic, with the greater diameter running obliquely; also dentate, stellate. It occurred in 20 cases bilaterally, and had but rarely reached the same stage of development in both eyes. This deeply dark disk was sometimes surrounded by

gray, irregularly formed specks, which, in one young lady, represented a second circular concentric disk in both eyes; in other cases by a large number of dark points of various size, but which never equalled the nucleiform cataract, and were sometimes very irregularly placed, sometimes appearing, more or less clearly, as a concentric layer, sometimes as a vicular, or as a stellar stratum. We saw these dark points, also, in those three individuals in whom the nucleiform cataract was wanting. In seven cases an opacity was seen at the outer border of the lens, which twice was encircled by a ring accurately defined and circular within, once by two concentric rings, separated from one another by a clear line, and consisting of thickly crowded points, while four times dentations were sent out towards the middle of the lens, by which the existing nucleiform cataract was rendered dentate or stellate.

The examination of more perfectly developed opacities of the lens, which may be already diagnosticated by the mere sight, shows that such cataracts always have a greater circumference than one would suppose from an exterior view. Here the speculum can accomplish something in the determination of the time at which the cataract will attain sufficient maturity for operation, since we have, in the visible progress of the opacity, a sure guide independent of the self-delusions of patients. In the examination of such strongly developed opacities of the lens, at times a deep black figure appears to us, which consists of three radii proceeding from the pole of the lens, growing smaller towards the equator; their direction corresponds exactly to the meridian lines, and there is no doubt that we have to do in such cases with the cleaving of these, often observed in cataractous lenses. Since the meridians of the anterior and posterior hemispheres of the lens have an opposite course, so we can easily determine from the course of these dark radii in which hemisphere the cleaving has taken place.

For the exact history of the development of cataracts, the speculum will be of the greatest use. Already it is proved with greater certainty that most opacities of the lens, taking their origin in the nucleus, extend in a centrifugal direction. This centrifugal extension is at times met by a second opacity, beginning at the border of the lens, and progressing in a centrifugal direction; but the latter is exceedingly rare. The opacities of the lens are composed of single dark atoms, which increase in number, become thickly crowded together, and at last conglomerate to a conformable mass. This conglomeration often proceeds with a certain regularity, and two special forms of it may be observed, namely, either these dark atoms unite to form concentric rings, or radiate lines. Only the opacity of the nucleus of the lens has appeared to us, hitherto, always conformable; probably the cause of this lies in the denser stratification of the fibres which exist in the nucleus, and the want of abundant uniting tissue. If the specu-

lum enables us on the one hand to recognize the least beginnings of the cataract formation, so also, on the other hand, it often teaches us that there is no opacity of the lens existing, when, from the mere sight, we should conclude that such was the fact. The pupil, especially in older persons, is not a pure black; it appears gray-yellow, whitish-yellow. Several cases have occurred to me, in which physicians had declared to eye-patients that they had a cataract, and might be eventually cured by operation, when the speculum proved that there was absolutely no opacity of the lens existing, and that the disturbance of vision was dependant upon a very different lesion of the eye.

Diseases of the Corpus Vitreum.—The circumstance that the vitreous body is almost completely withdrawn from observation, without illumination of the background of the eye, had made the diagnosis of its diseased conditions impossible. The disturbances of vision dependant upon this body, on account of the want of all externally perceptible alterations, were referred to lesions of the retina, and placed in the Augean stable of amblyopia and amaurosis. The speculum now teaches us, that diseases of the vitreous body, especially obscurities of the same, occur no more rarely than in the lens. These obscurities are of a twofold character:—either the vitreous body loses its natural transparency, the capacity to transmit rays of light to the retina, in which case, by the application of a convex lens, No. 6, the background of the eye appears wholly or in greater part very dimly illuminated, and the retina-objects to be found behind the obscured spots, as well as the vessels and papilla of the optic nerve, are either not at all or only very indeterminately made visible by the concave lens; or black corpuscles are found floating in this body.

The first, from the analogy of opacities of the lens, we must consider as true obscurations of the vitreous body; they appear to us like cloudy opacities, which have a diffused extent, and almost always involved the greater part of the vitreous body. In sixteen individuals, six of whom were completely blind, and the rest all suffering from important disturbances of vision, we diagnosticated this diffuse obscurations, since the retina-objects could not be perceived by any concave lens, and the background of the eye appeared dim, as if washed away. A confounding with opacities of the lens could not occur here, since cataracts, which so materially obstruct the vision, could scarcely escape an exterior view, to say nothing of the examination by the speculum. Besides, in five cases there were simultaneously slight opacities of the lens, which, however, appeared as small points, and could by no means explain the above symptoms. The conclusion may be drawn, that obscurations of the vitreous body of relatively slight intensity produce serious injury to vision, whilst cataractous lenses, which show ex-

teriorly a tolerably intensive gray color, often allow the patient the reading of coarser prints.

Still more frequently than these obscurations, we observe those dark floating corpuscles in the vitreous body, which are set in lively motion by the slightest oscillations of the globe of the eye, and cross before the clear, shining pupil in the most different directions. These corpuscles present the most manifold forms; sometimes they resemble small coiled serpents, sometimes polyhedric cells, sometimes long, irregularly formed coagula, sometimes they appear as innumerable floating points. We observed, also, the most various forms in the same eye, and, indeed, sometimes in such great number, that the vitreous body, after a movement of the globe, appeared like dirty swamp-water. When the globe returned to rest, these corpuscles sank to the bottom from the force of gravitation. We made this observation upon 26 individuals; four of them had normal sight; the most, however, complained to us of their own accord that they saw dark bodies floating in the air, and the description which they gave of their form frequently agreed very exactly with that observed through the speculum. They were often very short-sighted, so that they required acute concave spectacles. The vessels of the retina and choroidea were mostly clearly visible, nevertheless, we were obliged to use strong concave glasses, which corresponded well with the nearness of vision.

Concerning the nature of these corpuscles, nothing definite can be said; pathological anatomy and microscopy must furnish an explanation of them. We have not, hitherto, detected in them a spontaneous motion, so that we cannot at least consider them *living* entozoa. They may, indeed, often be blood or exudation-coagula; and the circumstance that in two cases we found a tolerably extensive extravasation of blood upon the retina, favors this view in some measure. From their exterior form, we might often also be led to consider them as cells; but it would be remarkable that such cells should remain so many months in the same stage of development. Perhaps they are many times residua of the lamellæ of the vitreous body, which, according to Bowman, exist in early life and later are broken up.

The frequent, manifestly swift movement of these corpuscles in the substance of the vitreous body, which, according to Kölliker's latest investigations, consists, in adults, of a more or less consistent mucus, permits us, nevertheless, to decide upon the fluidification of this body with some certainty, which accords also with the short-sightedness of such patients so frequently observed by us. Fluidification of the vitreous body makes its coefficients of refraction smaller; but this is not sufficient to explain the frequent high degree of nearness of vision, if we do not admit that through this means the diameters of incurvation of the refracting media are altered. In one man, who complained that upon motion of the eye he saw small,

clear, shining corpuscles floating about in the air, I observed in the vitreous body little glittering points, visible sometimes here, sometimes there, which disappeared upon rest of the globe. Could they have been crystals of cholesterine?

Diseases of the Retina and Choroidea.—The background of the eye appears to us, as we have already described above, as a field of vision shining with a reddish light, intercrossed by larger superficial vessels of the retina sending out single smaller branches, and by a deeper lying convolute of smaller vessels of the choroidea, upon which the *Papilla nervi optici* is accurately delineated through its intense brightness and the central vessels so clearly visible upon it. The truly surprising clearness with which all this is seen, did not allow Helmholtz to doubt that vascular distentions, varicosities, exudations before the retina, in its substance, and between the retina and choroidea, would be easily recognized. The observations hitherto made public have dispelled every doubt; and we also have not rarely observed pathological processes of the retina and choroidea. If we have not been able hitherto to recognize all visible abnormalities in their true essence, if we even overlook much which is abnormal, nevertheless, we can already assert that a large number of amauroses, which have heretofore been considered as neuroses, depend upon visible textural alterations of the retina and choroidea.

Most frequently we have observed distention of the vessels of the retina, by which their main trunks appeared enlarged, sent out many branchlets not formerly visible, and the whole background of the eye appeared of an unusually intense red color. It is true, that the size of the vessels, the number of their branchlets, the color of the background, are very different in different men, and, therefore, error is very easy; but the correctness of the diagnosis is favored on the one hand by the subjective symptoms, the pain in the eye and frontal region, the feeling of unusual fulness in the eye; on the other hand, by the frequent favorable result of treatment by the abstraction of blood.

The true inflammation of the retina with exudation seems also not to be rare. The spots covered with exudation appear, sometimes, whitish, reflecting the light strongly; sometimes reddish; sometimes they have a more greenish tint, and when of greater extent, are surrounded by an irregular dark border; sometimes, especially upon the *papilla*, they appear as dark specks. The exudation-mass, for the most part, encloses the vessels of the retina in such a manner that they become completely invisible, or at least appear only very dimly as white-red, not well defined marks. Only twice have I seen tolerably clearly the vessels of the retina running over the exudation-mass. In one case, already alluded to above, in which the ves-

sels of the retina were surrounded by a reddish exudation, we observed simultaneously in the vitreous body those coagula-like corpuscles, which probably had become loosened from the floor of the retina, and floated free in the vitreous body.

Injuries of the retina and its vessels from external violence very rarely occur, and are then mostly connected with such important lesions of the whole bulb, that examination with the speculum can furnish no results; it may also happen that a cataract needle in unskilful hands, or the lens dislocated by it, injure the retina. We have not observed cases of this kind. Van Trigt has wounded the internal eye of cats, dogs, and other canines, with needles, and in this manner made highly interesting observations upon the results of these injuries and the cicatrization of the wounds. On the other hand, we have observed a case of spontaneous rupture of the central vessel, which I may be allowed to report in detail.

A man, 55 years of age, plethoric, was suddenly seized with giddiness, and sank into unconsciousness. As he aroused from this condition after an hour, he was blind in his right eye, which before was perfectly normal. The physician called first, made a venesection, and otherwise conducted the treatment *lege artis*. Upon the fifth day after this mishap, he came to my office to consult me on account of his eye. The pupil of the right eye was more strongly dilated than that of the normal left eye; both had a yellowish grey mucus, otherwise nothing abnormal exteriorly. The examination with the speculum gave the following result:—the *papilla nervi optici* was accurately bounded on the outer side, nevertheless even here was more dimly illuminated than usual; at the inner and inferior side it was covered with a red blood-coagulum. From the upper border descended a vessel, which, irregularly dentated, terminated free; all the other vessels of the *papilla* were covered with the coagulum. Otherwise all is normal. There could be no doubt that here a laceration of the central vessel had taken place at the point where it issues from the *porus nervi optici*. Ten days later, the coagulum, now of a less dark red, was still visible only at the inner border; all the rest of the *papilla* was of a greenish color, with occasional clear spots interspersed; out of its midst was now seen passing downwards a rose-colored, not well defined line, which glimmered but dimly, and at its superior border vessels were distinguished which represented two arches. In the vitreous body floated single dark specks. The power of vision continued totally extinguished.*

Morbid dilatation of the vascular walls we have never observed except in veins, which then appeared like dark, undulatory or spiral cords running over the field of vision. We never found them upon the *papilla*.

In a young man, who saw objects perfectly clearly only in a bright sunlight, but recognized them very imperfectly with a cloudy heaven, at even-

ing, and by lamplight, we found the retina covered round about the *papilla*, here and there, with black specks; the *papilla* itself likewise contained several dark specks, but appearing less black, and less accurately circumscribed. Otherwise nothing abnormal. The supposition that here a pigment-transudation had taken place, sufficiently explains the perfectly normal perceptibility of the patient existing only in an intense light.

In conclusion, I will make mention of one other case, in which the speculum showed us, with tolerable certainty, that a lesion of the optic nerve itself was the cause of the blindness.

F. S., 11 years of age, with brown hair, brown-yellow iris, and very dilated pupils, was born amaurotic in the left eye; the right eye normal. The left ocular globe is a little smaller than the right, and sits deeper in the orbit. The *papilla nervi optici* appears as an unusually small elliptic disk whose lesser horizontal diameter is about half as large as the great perpendicular diameter; the *arteria* and *vena centralis* give off as usual two branches, but very fine, and besides the branchlet passing inwards, a second is also here visible running outwards; the *macula lutea* and the rest of the background of the eye present nothing abnormal. By means of a concave lens, No. 3, we discovered a central punctiform opacity of the lens. The right *papilla* is circular and quite large. While here, on the one hand, the circumstance that the amaurosis of the left eye was congenital, and on the other the obstructed development of the bulb warranted us in concluding upon an analogous condition of the optic nerve, so also the examination with the speculum confirmed this diagnosis.

Sanguinous distention of the choroidal vessels is a very frequent phenomenon, which is characterized by abnormal size and intensive dark color of the choroidal veins. We have also observed, in many cases, genuine choroiditis with secretion of the exudation mass. When the exudation is deposited in small circumscribed spots, the background of the eye appears uneven, and reflects the light from those spots stronger than from others; if the exudation has a greater extent, the retina becomes mostly separated, as it were, at the point of entrance of the optic nerve, and projects like a large eminence, whilst it crowds the *corpus vitreum* out of its position; such an eye has then lost the normal relation of internal parts, and the vessels coursing upon the hill-like tumor may be seen by mere illumination from the mirror without the application of a concave lens. Since such extensive exudations are mostly serous, of a fluid nature, so the retina, together with the vessels, may be seen floating after a movement of the globe. Ruptures of the retina from exudations crowding upon it from behind, in consequence of which it floats about freely in the exudation and disorganized vitreous body, we have not observed. In one case of choroiditis, which had had its seat more in the region of the *ova serrata*, we found the following. If we

looked through a convex glass, No. 6, in the direction of the axis of the eye, the pupil being nearly normally illuminated, a dark body appeared at the lower border of the papilla, which was bounded above by three convexities, and floated hither and thither upon motion of the bulb, without leaving the border of the iris; simultaneously there appeared several other dark dentations, also black coagula-like corpuscles floating free. If we looked inward, the eye being directed inferiorly, we saw behind this dark body a yellowish green exudation mass projecting inwards deep into the eye, which formed several strata lying behind one another, representing mountains as viewed from a distance. Through a concave lens we beheld, in the very depth of the eye, other well-defined black specks; but the vessels were seen only very imperfectly with the eye directed strongly upwards. The exudation, in this case, penetrated the vitreous body.

If we now review the truly surprising results which the *speculum oculi* has furnished us in so short a time, we can not only concur in the expectation cherished by the highly esteemed inventor, that all the alterations of the transparent media, the retina, and choroidea, found in the corpse, will also be recognised in the living eye; but also assert, with safety, that it will be possible for us now to pursue with exactitude the progressive development of the pathological processes of these structures, as well as their recession, whether effected by nature or not.

The advantages which will be derived by the practising physician, irrespective of the cure of these diseases, we have not hitherto considered; but one thing is certain, that an exact knowledge of the diseased objects must be the basis of a rational therapia: without this knowledge we grope in the dark. We shall not be able to cure all diseases, even in this manner; but the physician gains a great advantage thereby, when, from his knowledge of their incurability, he holds himself aloof from fruitless, perhaps injurious, attempts to cure.

PART IV.—HOSPITAL RECORDS.

REPORTS.

The writer of this department claimed the privilege, in the last issue of the MONTHLY, of commenting upon a passage in the article which appeared in No. 3, entitled "Dr. March and his Reviewer," over the signature of Louis Bäuer, M. D., &c. The passage referred to is as follows: "I never heard of any cure having been consummated in this way, until I happened

to read No. 1 of the A. M. Monthly. It is stated therein (vide Hospital Records) that by encouraging the movements of the affected limb, Dr. Carnochan had succeeded in removing the deformity, and restoring the utility of the limb. Incomprehensible as these startling results appear to a surgeon who has acquired some experience on the subject, nevertheless—'Brutus says it, and Brutus is an honorable man.'

There are several points covertly made in this sentence, which, for its purpose, is admirably constructed, and renders superfluous the apologetic strain of humility with which Dr. Bäuer affects to put forth his literary crusade in a language foreign to him. It is intended, firstly, to imply that the writer of the Hospital Report was ignorant of the subject of which he was treating.

Secondly, to deny and disparage Dr. Carnochan's success in the treatment of morbus coxarius.

Thirdly, to imply misrepresentation as to matter of fact; and,

Fourthly, to ridicule the whole statement as opposed to his (Dr. Bäuer's) peculiar views on the pathology and great experience in the treatment of this particular form of disease. With the first, third, and fourth points, it is my present purpose to deal; and of these only is it necessary, or would it be decorous, that I should treat. The second involves a matter of wide range and great importance in a practical point of view; but, inasmuch as it bears principally on a third party, who is perfectly competent to fight his own battles, and who would most probably neither desire nor encourage any advocacy, were I disposed or competent to attempt it, I can, very consistently, and most undoubtedly will, leave him to select his own time and manner of reply.

Disliking extremely, and wishing to avoid the empirical method of "blowing one's own trumpet," I shall pass over, with brief comment, the first point. It will suffice to say that a careful perusal of the principal standard surgical works, of a fair proportion of the special literature of the subject, many frequent opportunities of witnessing the treatment adopted by various practitioners, and the professional care of a few cases during a practice of nearly twenty years, would seem to justify the expression of an opinion on the phenomena presented by patients under treatment, wherever seen.

With reference to the third point, I shall be less scrupulous.

In the performance of my duty, as the contributor of the Hospital Reports to the pages of this periodical, it is my aim and strong desire faithfully to describe every thing which I see, and chronicle all that I hear, which, in my humble judgment, appears to possess sufficient interest for the professional reader and the practitioner. In accomplishing this aim, and in fulfilling this desire, I exercise, to the best of my ability, my powers of

observation and the faculty of memory. Sharing largely in the fallibility of human nature, it is possible, and very probable, that I may err in judgment as to the value of a particular fact observed or heard in relation, and so may sometimes give place to cases which to other minds will appear indifferent and unimportant. It may even occur that I shall omit to record some circumstance or statement, through inadvertence or lapse of memory, which would be valuable. But of this the readers of the MONTHLY may rest assured, that whatever I do relate shall be a true and correct report of what I have either personally witnessed or heard related. And that when I venture upon an opinion, it will be the result of conviction in my own mind, and not the reiterated views of another.

It appears from a foot-note by the Editor, attached to the quoted statement, viz.: that Dr. Carnochan "encouraged motion of the affected limb," that, at the time the report appeared, Dr. Carnochan objected to this assertion, as not conveying a correct impression of his mode of treatment. The explanation given being, Dr. C. "allows," *i. e.*: that he does not object to, but that he does not encourage motion. I regret that I should have failed to apprehend sufficiently this nice distinction, and I must acknowledge, that I do not as yet fully appreciate its force. In justification of the conclusion at which I arrived, I will state the premises on which I reasoned that encouragement was given to the movement. Fortunately, these premises are not hypothetical. On visiting the wards of the Hospital in which the patients are, on more than one occasion, they were requested to walk across the floor. In some, it is true, this was done to exhibit the perfection of the cure; but in others, at different stages of the progress of the disease, to prove the absence of all restraint to the movement of the joint. I do not consider, however, that this misapprehension, if such it really be on my part, affects the general principle of Dr. Carnochan's mode of treatment, nor does it detract from the accuracy of the report, as regards the result—the main point, after all. Let me try if I can be more successful in reporting accurately what I witnessed in Dr. Bäuer's Institution, on the only occasion on which I have had the opportunity of seeing the patients under treatment in his wards.

I will premise that I concur, in many points, with the views entertained by some of those authors from whom Dr. Bäuer has formed his own theory on the pathology of this disease, but on points of practice, as laid down by them, and adopted and insisted on by him, I differ essentially. There is no doubt that he has taken great pains to render himself intimately familiar with the literature of the subject, and has given much attention to this speciality of practice. To reason upon, theorize, and put in force his therapeutic and operative surgery, he is well qualified—but this does not make him right. Of his measure of success I have no means of determining.

There were, at the time of my visit, only two patients in the house. Both young, I should think under eight years of age. One, a boy, was lying on his back, with the head and shoulders considerably below the level of the hips, which were placed on an elevated padding of some description. To the best of my recollection, the left hip was the affected one, and that extremity, bent over this saddle, so to speak, was evidently forcibly extended, and kept so by an arrangement of straps and adjusting screws, or rack-work. In this position the little sufferer had been kept for some two or three days previously. The leg was cedematous; the outline of the extended muscles on the anterior aspect of the thigh, the dark tracing of the integumental and subcutaneous veins were evidences of the mechanical force exerted to maintain this position of the limb. The whole attitude of the body was one indicative of irksome constraint, to me, at least, painful to witness. The pale transparent skin, the feverish flush on the cheek, the saddened expression of features, and the mournful, wistful gaze of the eye, created an impression little calculated to induce a preference of this mode of treatment.

The other patient, a little girl, was lying on the bed on the chest and abdomen, her face directed towards some toys placed for her amusement, an expression of grateful relief in marked contrast to the boy's. Treatment had been successful with her, I was informed, in restoring the length and normal position of the limb; she was then undergoing treatment by poultices on the sacral region, for a boil, as I understood.

It is but right to state that my visit was paid shortly after the commencement of the doctor's institution, and therefore the limited opportunity for observation afforded. But, judging by the appearance of this case, I frankly confess that my objection to, or it may possibly be prejudice against, the treatment, by extension and counter-extension previously existing, was fully confirmed. I will not presume to say that it is unwarrantable, or even unscientific, but it certainly appears objectionable to submit a delicate frame to such restraint when other means are found to answer an equally efficient purpose at a less cost to the constitutional powers, even if this be but temporary.

Let the reader of Dr. Bäuer's caustic article, in which he so recklessly assails an unoffending reporter, consider well the force of his prefatory exordium on the qualities of criticism and the duties of a reviewer, and, judging the remainder of that production by the standard of perfection and excellence there established, pronounce whether he has abided by the rules laid down. Alas! poor human nature; why is it that, with all the accumulated experience of centuries to guide us, the daily recurrence of glaring examples to warn us, the advantage of a familiar knowledge of the broad distinction between them, and the freedom of choice, we are constantly falling away from what is right, and wilfully wandering into the fields of error—of wrong. Stick

to your principles, Doctor; you will have enough to do to maintain those, when fairly engaged in the controversy you seek to provoke. And who is he, may fairly be asked, who assumes* to decide thus peremptorily on the merits of this or that course of practice. A "surgeon, of some experience," I may be prepared to admit; an earnest seeker after scientific truth and knowledge, I will believe; but what has Dr. Bäuer done to justify a classification of his own with names like those of Petit, Boyer, Larrey, and Brodie. Beyond one or two contributions to a contemporary periodical on his favorite subject, and the issue of an expository pamphlet with reference to his private institution in Brooklyn, we know of nothing which entitles him to the rank of an authority in general or even special surgery.

The egotism of a writer, if not very skilfully managed, will always act as a hazy mist, dimming the lustre of his genius and detracting from the value of his labors. In the present instance, the evil of failure in this management is but too apparent. The gratuitous defence of Dr. March, the unjustifiable assault on Dr. Carnochan, the heedless, unfair, and uncalled for attack on the writer, the self-established association of names,† and the prominence given to the B. O. I., all tend to induce the conviction that the lust of notoriety may be as strong as the love of science. This reply to the doctor's fierce attack may possibly contribute to the end in view; but, despite such a result, I felt it due to myself to parry the thrust, and only hope that the readers of the MONTHLY will excuse the prominence given to the subject.

There really has been comparatively little of interest in the practice of the last month, calling for comment. But, in the mortuary returns of the city, the number of deaths occurring from croup during the two last weeks of March, and the first two of April, is somewhat remarkable. The average exceeds that of former years considerably, and it would be a fruitful topic of investigation to inquire into the causes operating to produce this increase. Of recent years, much light has been thrown on the pathology of croup, and the treatment has undergone many modifications in accordance with our improved knowledge. Much prejudice still exists against the method proposed and successfully practiced by Dr. Greene, of applying the caustic solution to the internal surface of the larynx and trachea; but evidence of its utility daily multiplies, and we are convinced that ere long it will supersede the operation of tracheotomy, and displace entirely much of the exhaustive constitutional treatment hitherto regarded as essential to the cure.

* As this is a word apparently most repugnant to the Doctor, I venture to explain it as meaning in this connection, "who, of his own accord, takes upon himself."

† Vide page 191, "Bonnet, Lorinser, Buehring, March, myself, and others," page 195, "Bonnet, Buehring, Lorinser, Malgaigne, myself, and others."

PART V.—EDITORIAL AND MISCELLANEOUS.

THE STUDY OF ANATOMY.—By a postscript appended to the April number of the MONTHLY, we announced to our readers that the bill legalizing dissections had passed the Legislature of the State. It has since been signed by the Governor, and has become a law of the Commonwealth.

No law could be passed, we are sure, which would afford to the whole profession of the State so profound gratification, or which could be more justly felt to be that which every sentiment demands. Its necessity is apparent from the almost universal demand made for it by the whole body of physicians, and the general effort made in favor of the bill by those in different parts of the State. We may say, without impropriety, that the MONTHLY contributed its full share to these efforts; and we have some reason to believe that the republication of Macaulay's able argument upon the subject in our pages was both seasonable and influential. This same speech, by the by, we would commend to the perusal of gentlemen of learned societies, before they attempt to put in a claim for New York as the pioneer in this humane enactment.

This much we should not have said, chiefly for want of a fitting opportunity, had not our attention been particularly called to the matter by two papers, emanating from two entirely different sources, but both characterized by so much that is erroneous and injurious, that we are not at liberty to pass them by. The first is of consequence from the high position of its author, and the popular magazine in which it appears; the second from its having emanated from certain gentlemen in the profession.

To speak of them, then, in succession: the former appeared in the April number of Harper's Magazine, under the heading "Editor's Table," the title of the article being "The Sacredness of the Human Body." It is understood that the article was written by Professor Tayler Lewis, LL. D., a gentleman whose name is familiar not only in discussions of the Platonic Theology, but in many other departments of science and literature. It is hardly necessary to be said that his name carries with it a great weight of authority; and though one may with diffidence oppose so far-famed a knight, yet the double arming of truth and right gives irresistible strength.

There seems to have been a double incitement for Professor Lewis' article—the one the threatened invasion of Trinity Churchyard in this city, by the construction of a street through a thousand graves; the other, the application for the passage of the Anatomy Bill,—or, to use his own words, "the claim of science and the claim of the mart." The learned writer, therefore, commences his dissertation by stating some allowed truisms con-

cerning the respect which in all ages, and among all nations, has been felt for the remains of the deceased, and puts the question whether or not this is a prejudice and superstition, or is grounded in reason as well as in the purest moral and religious sentiments. He then continues—

“The sacrilegious tendency of which we speak, shows itself in two ways. There is the claim of science, and the claim of the mart. The doctor and the merchant both demand that ‘the earth shall give up to them her dead.’ One wants the bodies themselves; the other, the room they occupy. One presents the plea of useful knowledge, the other of increasing trade which must have its accommodation. One would extract the elixir of life and health from mortality; the other would create the philosopher’s stone from dust and ashes; it would bring out of dead men’s bones that charm of the alchemist which shall convert the vaults of the church-yard into the vaults of the bank, and the moss-grown, mouldering monuments into the bright gold of an advancing commerce. It must be confessed, however, that there is no little inconsistency in the respective attitudes of the two parties so clamorous for a similar object. Some have not hesitated to advocate the claim of the doctors, and to talk of the necessities of science, who have been horrified at the thought that the sacrilegious foot of trade should disturb the bones of their ancestors. The hospital, the almshouse, and the prison, may thus be invaded for the secular good of humanity, but Trinity ground is sacred. There are higher associations there. The church, too, comes to the rescue, and nobly must we say has she fulfilled her duty in the case.” * * * * * “The church is in the right. We praise her pious zeal. Every other Christian church, or denomination, in the land ought to make common cause with her. Yet still, must we say it, the other invasion of the dead is no less sacrilegious, while it makes even a deeper appeal to our human sympathies.”

Such a pitiful begging of the question is not worthy of so learned a man. About to show, as he believes, that all interference with the bodies of the dead is contrary to instinct and revelation, he commences by asserting it to be sacrilegious. Then, too, he proceeds to conjure up all those feelings which, however natural they may be, are not the arguments of the logician.

He then continues—

“We can not help thinking that the necessities of medical science have been greatly overrated. Even where the want is conceded, the benefits may be purchased too dear. Better that the causes of some bodily diseases remain concealed, than that the knowledge of them be obtained at the sacrifice of some of the best feelings of the soul. But, admitting the force of every plea, may we not ask—is there not in many cases, in most cases, perhaps, an unfeeling waste?” * * * * * “We would be cautious here in treating of a matter which, it may be said, the writer does not professionally understand; but must it not strike almost every unprofessional mind in the same light? Why this apparently enormous waste? Why must the human body be dissected over and over again ten thousand times, not so much for the discovery of new truths—for that is not even alleged as the ground in most cases—but to explain old and well-known truths to

every new class of students? May there not be made most accurate anatomical representations by means of drawings, by preparations in wax, and other modes that might be mentioned, reserving dissections for those cases alone where the parts are too minute and the action too microscopical to be set forth by any such methods? Can not a knowledge of the general anatomy be given unless a man is cut up every time the class comes before the lecturer? These questions may perhaps betray ignorance of the subject in some respects, but of the ordinary workings of human nature all intelligent men are alike judges, and upon the minds of such the conviction will press itself, that the hardening effect of these scientific butcheries—we mean to use the term in no more offensive sense than if we were applying it to the worthy citizens who supply us with animal food—must produce an indifference, a recklessness, which not only leads to the waste of which complaint is made, but actually comes to believe it indispensable! The right feeling on this subject might, perhaps, obtain results equally scientific, and equally valuable, from far less means, and with far less sacrifice of what is of more value than almost any amount of knowledge, whether speculative or practical."

We have thus quoted freely as much as our limits will allow, in order that we may not be thought to misrepresent the style and manner of Prof. Lewis' argument. We may here add that the whole is in the same strain.

In the first place, we say, deliberately, that these questions do certainly "betray ignorance of the subject in some respects;" and though this is surprising in a man who claims to be educated, it becomes highly *culpable* in one who sets himself forward to teach the people. Were his views called for and their utterance insisted upon, it would be different; but he has selected this from the thousand topics fitted for a popular journal, and has entered upon its discussion uninformed and ignorant. Would Professor Lewis consider a person, who confesses his ignorance of the philosophy of Plato, fitted to thrust himself forward to instruct the people upon its claims and its teachings?

What are the *facts* upon this subject, and what are the necessities for continual dissections? To state them briefly, they are, that there is continual occasion for the performance of surgical operations that life may be saved; that these operations may be required at the hands of any member of the medical profession without a moment's notice, and without a moment's opportunity to consult books or friends; that the knowledge requisite for their performance cannot be obtained from books, or plates, or preparations, or models, and is only to be acquired by each individual, who is an applicant to be admitted to the medical profession, first obtaining it for himself; that by means of the information thus derived, as one of the essential requisites, the average duration of human life in civilized countries has been materially lengthened, while diseases before incurable are found to yield to the deductions of scientific observation.

To illustrate each of these statements would be easy ; but our space does not permit it.

But, says Professor Lewis, "even when the want is conceded, the benefits may be purchased too dear. Better that the causes of some bodily diseases remain concealed, than that knowledge of them be obtained at the sacrifice of some of the best feelings of the soul." Admit, for a moment, both these propositions, and it is not at all clear that *now* either the benefits are purchased too dear, or that this *sacrifice* is made. In fact, quite the reverse is true ; and still a popular writer, not possessing especial knowledge upon the subject, indirectly asserts this to be the case.

The remainder of our writer's argument, if we may apply the word to a production in which there is neither reason nor logic, is as follows. Though dissections do no hurt to the dead, they do an immense injury to the living.

"Every thing, therefore, is unchristian, as well irreligious and demoralizing, which goes to destroy any feeling or association of ideas so vitally connected with this great truth of revelation (the resurrection of the body). The air of the dissecting room is unfavorable to it ; not that the superficial scalpel of the surgeon could ever penetrate the psychological and physiological mystery that lies so far beneath, and thus show the falsity of the common belief ; but the outward appearances, the outward, material, tangible associations, are hostile to the scriptural view."

Of this position we simply say it is incorrect in its premises, false in its facts, and unsound in its deductions. Still, of this the writer says, "and this is the point we wish mainly to present."

"Now what a contrast to all this religious feeling, so tender, so melancholy, and yet so full of moral health to the soul—what a contrast, we say, to these blessed influences that come to us from the grave in connection with the doctrine of the resurrection, is presented by the scenes and associations of a dissecting room—the sacred human body, the once loved form, the former temple of a loving spirit, thus lying mangled, debased, deformed, made the subject of unfeeling remark by some cold, materializing lecturer, and exposed to the rude gaze and ruder hands of hardened, and, it may be, licentious students."

Cold, materializing lecturer ! Licentious students ! By what authority does Professor Lewis thus calumniously speak ? But we will refer to this again.

Our author then proceeds to speak of the necessity for postmortem examinations, and that friends willingly consent to them, if the breach is so closed that the body again presents "that appearance of entirety which the conceptive faculty demands when we would think of the state of the dead." He then asserts that such a spirit does not pervade the dissecting room, which, if it did, would, he thinks, render that less objectionable.

Repeating, then, the stale objection to the appropriation of the bodies of those who are without relatives or friends, and which Macaulay has so thoroughly answered, he closes the portion of his paper which concerns us thus:—

“The medical profession, it is said, must have subjects. If so, let them be content with the fewest possible; let the most serious wisdom among us be exercised in providing the means with the least sacrifice of feeling, the least of moral detriment; and then let the necessary duty be ever discharged with all the devout reverence of a high and religious trust.”

Precisely; and it is simply because the medical profession have been endeavoring to accomplish this very thing, that this tirade has been poured out upon them.

The Anatomy Bill was proposed, has been urged, and is now accepted as a law for these reasons. However Professor Lewis may *think*, the members of the medical profession *know* that there is greater good to be done, not only to the bodies but, indirectly, to the souls of men, by the prolongation of human life resulting from increased and thorough knowledge of anatomy, than by the anxious preservation of that “*appearance* of entirety which the conceptive faculty demands.” For years they have obtained this knowledge, though unaided and opposed by ignorant law makers, and have conferred the boon of prolonged life upon rich and poor by themselves incurring the greatest risks.

Having found that, by a law appropriating to their use the bodies of a class who are without friends to suffer pain in consequence of their use, the science would be pursued “with the least sacrifice of feeling and the least of moral detriment,” they have proposed and obtained, in a few States, such an enactment. And in this they were right. The law was not desired that *more* subjects might be had, but that they might be obtained legally, and that this necessary pursuit might not bring with it the risk of ignominious punishment.

But we cannot dwell longer upon this.

A subject, forced upon us by the spirit of Professor Lewis’ article, must, however, occupy a little space. That so learned a man as he has repeated the stale slanders against our noble profession which are found in this article, is a painful thought. Neither are these things very uncommon among educated men. We could endure it from those who are not familiar with literature and history; but when Professor Lewis accuses the whole medical profession, as he has done, of materialism and scepticism; when the learned Dr. Cogswell, of the Astor Library, takes occasion to go out of his way, in describing that institution, to sneer at medical men for not working without a fee, when no class of men render so much service gratuitously, it is time that we repelled the charges publicly.

Professor Lewis asserts that the air of the dissecting room is unfavorable to a belief in the doctrine of the resurrection. The argument which would be necessary to render the error of this statement palpable, is simple, but for its illustration we must occupy more space than is at our command. We are compelled, therefore, to be content with asserting (and our assertion should be as authoritative as that of Professor Lewis) that there is no class of educated men who are, as a body, more sincere, firm, and intelligent believers in this great doctrine, than physicians. They, at least, understand that, when they say "I believe in the resurrection of the body," they do so because it is the teaching of the Scriptures, and not that of any human master. "With what body do they come," is not *their* question. It is in the sectarian class room, not in the anatomical theatre, that this inquiry is mooted. The man who studies by dissection the structure of the human body, too often meets evidences of design not to ask who is their designer—and too often meets ultimate facts beyond which he cannot go, not to learn to submit his reason to the confession that there are many things beyond it. In Professor Lewis' own words, "Fathers, and schoolmen, as well as modern metaphysicians, have filled volumes with arguments in respect to what constitutes bodily identity. Yet still—faith clings to the dogma and will not let it go." And no one's faith clings more closely, and less doubtfully, than that of the members of the medical profession.

It is not any more correct that we are materialists. Though we cannot discuss this topic, we will add that we can conceive of no better remedy for the mind folded in the cold, heartless embrace of materialism, than to pursue *thoroughly* the study of anatomy.

We do not claim that of our profession (we do not include in its ranks the half-educated and cheating empiric) all the members are free from scepticism and error. But what we do assert is, that they have not had, neither do they now have occasion to fear comparison, in these respects, with any class of the community; and therefore the jibes and sneers, so freely bestowed upon us by other educated men, are as undeserved as they are unkind. When men, as cultivated, as pure, as refined, as wise as any, claim that, for the study of their science and the good of the public health and morals, there is need of the study of anatomy, and point out the best way, in their opinion, for accomplishing it, it is neither in accordance with sound logic nor Christian principle for men of education and influence to point the long finger of scorn at them, and stir up the less-thinking populace by the cry of Ghouls, Vampires, and Infidels.

The second paper to which we referred in the commencement of this article is, a remonstrance sent to the Legislature of the State of Massachusetts, by a committee of the Faculty of the Massachusetts Medical College, against the application of the Boylston School, in Boston, for power to con-

fer degrees. To this application allusion has been made by our Boston correspondent, in his letter which we publish this month, and the merits of the case have been fairly stated by him. This remonstrance is, on the whole, a precious document, and we confess to having had a good amount of amusement over it. We cannot give it entire, but quote in succession some marginal notes, indicative of the intended force of the paragraphs against which they are set.

"Medical schools are of two kinds; 1 private, 2 colleges." We presume this discovery will be at once admitted to be true.

"Boylston, a private school. Tremont school larger." The Tremont school is taught by the Faculty of the Massachusetts Medical College, so that they are very disinterested parties in speaking of it.

"A new school is not wanted. Already eight." It is not very remarkable that the committee should suppose the first proposition to be correct, though the general opinion of the profession is hardly in accordance with it. The *eight* schools are those of New England, of which the committee say that they are "almost all in a languishing state." If connected with one of them we should request the committee to speak for themselves.

"Mass. Medical College, a *State* institution, well provided with means of instruction." The *text* dwells upon the value of the *apparatus* belonging to the school, and asserts that the incorporation of new schools "tends to entice students from places where they are well taught to places where they are liable to be worse taught." Rather modest, in this connection—but if our correspondent is correct, and we incline to think he is, students in Boston would not at all stand in this danger if the Boylston school is chartered.

"Now more physicians than the public need." If the committee think so they had better shut up their school.

"There are not anatomical subjects enough. Dangers." In the opposite paragraph the committee say, "In New York and Philadelphia, hundreds of bodies are every year stolen from their places of burial. In this commonwealth, the same violation of graves was notoriously carried on, until the passage by the Legislature of what is commonly called the *Anatomy Law*." This is a rather *cool* statement, to say the least, for men to make who are no more willing than other physicians to do without anatomical material in teaching students; and are quite *as* unscrupulous as are the teachers of New York and Philadelphia about stealing bodies. But it appears to be still *cooler* when it is known that they readily seek the aid of resurrectionists in this State to supply their deficiency, and are only too much provoked when from any reason they are disappointed in obtaining their supply. We really do hope that these virtuous gentlemen will not find their pure robes soiled by the contact of the brethren of

New York or Philadelphia. The "Dangers" are, in reality, that the committee fear a scanty supply of students, professedly they fear a still more scanty supply of subjects—and that this "will act as a premium on the illegal getting of subjects which is now unknown (?) in this city and State." We again salute this spotless virtue. The last marginal note is, "if this is chartered others must be so," which would of course tend still farther to injure the committee's institution. "This has not been the previous policy of the Legislature." Therefore it should not be changed.

But now, putting jesting aside, we ask if it is worthy of men who are prominent in New England, as teachers in our profession, thus to appeal to popular prejudice against the study of anatomy, in order to influence a legislative body against conferring a charter upon a rival institution to be conducted by gentlemen every way as honorable and as capable as themselves. It must not be surprising to this committee if, for any reason, a popular tumult should be excited against themselves or their institution, to be compelled to meet their own words and arguments. To accomplish their own immediate purposes they have assaulted their brethren, and, though assaults from strangers are not unexpected, this selfish treachery at home is shameful.

What the result of this application is, we have not definitely learned—but, for our own part, cordially hope it may be successful. Neither do we know the comparative strength of the parties—save from one indication, namely; that the medical periodical published in Boston has not spoken of the matter, from which we judge that they are nearly equal. When this takes a part we shall know who is strongest.

FROM OUR BOSTON CORRESPONDENT.

Massachusetts Medical College. Boylston School. Catalogue of the Hospital. Three cases of Hydrophobia.

Our quiet city has furnished even less than its usual quota of medical items during the past winter. At the Massachusetts Medical College the number of students has not exceeded the average of about one hundred; twenty degrees were conferred at the close of the term in March, with a less number to be conferred at the annual commencement of Harvard College in August.

Considerable discussion has been excited by the application of the Boylston Medical School, for permission from the legislature to grant the degree of doctor of medicine. Dr. Winslow Lewis, whose name stands at the head of the School, has long been known to the profession in New England, as

among the most skillful and experienced of its surgeons. Other members of the school have been distinguished among us, for their long and successful practice of their respective branches, and the thorough and faithful discharge of public professional offices in the city, or else as young men whose professional education, in the best schools of our own country and in Europe, is a guarantee of their ability for the post of Professor. If successful in their petition, they propose to make important modifications in the arrangement of lectures and recitations, after the model of the French and German schools, by extending the term through the year, and giving to each student a systematic course of instruction for the three years of study. As a guaranty for their standard of instruction, they propose to submit their graduating classes to the examination and judgment of a committee of the State Medical Society, instead of the Professors themselves, in this way avoiding the much-dreaded depreciation in medical education. Any one who is at all familiar with the wants and peculiarities of medical students must, it seems, have felt the necessity of some change in the manner of medical instruction, so far inferior among us to the standard in other countries. And the feeling among physicians here is decidedly in favor of the petitioners; that, if they are willing to take upon themselves such an arduous duty in the face of the prestige of Harvard University, they deserve the best wishes of the Profession.

The Trustees of the Massachusetts Gen. Hospital lately ordered a complete catalogue *raisonné* to be made of all the surgical cases which have been treated in that institution since its establishment. A daily record has always been kept of each surgical visit, and afterwards copied into a volume. These have been accumulating for a large number of years, embodying an invaluable amount of clinical observation, but of little use to the student on account of the diffusion of each particular class of cases through a large number of volumes. The catalogue is written in the largest-size folio volumes; the diseases are arranged and subdivided in alphabetical order; the page is ruled perpendicularly into spaces specifying the name of the surgeon in attendance, the kind of ether given when anæsthetic agents were used, the age and sex of the patients, the disease, general course of treatment or operation, the duration of the case, the result, and the volume and page of the general records in which the minute daily details of the case are given. These records extend through a period of thirty-three years, in one of the largest hospitals in the country; every case has been observed by men whose names are a sufficient voucher for the correctness of any diagnosis or the success of any operation. The value of this long series of clinical observations, so carefully classified, can hardly be estimated.

Within a few months three well-marked and carefully observed cases of hydrophobia have occurred in our immediate vicinity, affording an oppor-

tunity for direct comparison of the progress and symptoms of that rare disease, which is not often met with. The first case is reported in the Boston Medical and Surgical Journal, by Dr. Geo. Hayward. The subject was a lad, seven years old, who was bitten in the month of August, 1853. The principal wound was on the upper eye-lid. There was another, more superficial, near the angle of the mouth on the opposite side. One hour afterwards, the wounds were sucked by the attending physician for two hours (the position of the principal wound rendering excision or cupping impossible). They were afterwards thoroughly cauterized with nitrate of silver; the wounds healed kindly, and the lad appeared in perfect health for a month, with the exception of some sensitiveness to cold, which, however, was not noticed at the time. Monday, Sept. 12th, thirty days after the accident, he passed a restless, uneasy night, complaining of his stomach. Tuesday morning he had no appetite, said he was thirsty and wanted water, when it was brought toward him he seemed agitated; when carried nearer he was slightly convulsed; and on approaching it to his lips he cried out in great terror. He complained at that time of pain in the eye where he was bitten, but there was neither redness, swelling, nor tenderness about the cicatrix; he was unable to swallow medicine; was very sensitive to currents of cold air; the skin was hot and dry; pulse rapid; respiration hurried; and the mouth was filled with frothy saliva. After a sleepless night, on Wednesday the symptoms assumed a graver character; he walked about the room in a wild, impatient way, carrying his head on one side; seemed much disturbed by the entrance of visitors; when asked what was the matter, he placed his hand on his throat and said he could not swallow. His respiration was hurried so much as to interrupt utterance at almost every word; a full respiration was followed almost always by a violent convulsion. Giving him a small piece of bread, he put it in his mouth, but was unable to swallow it, though making the strongest efforts to do so; the pulse was 120, and the respirations more than 40 in the minute. On Thursday, the third day, partial hemiplegia supervened, his speech became indistinct, but as long as it was intelligible he appeared perfectly rational; he died about 11 o'clock that night; no active treatment was resorted to.

The second case was reported by Dr. John D. Homans to the Society for Medical Improvement. The patient was a boy, fifteen years of age, who was bitten Jan. 27th, 1854, in the right ear and the calf of the leg; the wounds were washed with laudanum and water, and healed without trouble. He seemed in usual health, except a slight elation of spirits, until Sunday morning, Feb. 25th, twenty-nine days after the accident, when he was observed to shiver, and said "there was a catch in his breath every few minutes." In the course of the day he became unable to swallow liquids without great suffering. He slept little during the night, and on Monday

was found in bed, with hot, dry skin, furred tongue, eyes wide open, with a vacant expression, pulse 80-100, irregular and intermittent. Touching him suddenly or speaking quickly to him produced a shuddering of the whole frame, with muscular spasm. He was not inclined to converse. There was great thirst, though he had not been able to swallow any thing for eighteen hours, from the spasm of the throat. The skin was bathed with an acid, offensive perspiration. By approaching the mouth gradually with the teaspoon, waiting till the spasm produced by the sight of the fluid was over, and then turning the contents quickly into the mouth, he was able, with great difficulty, to swallow a small amount of liquid. These symptoms increased rapidly in severity during the day and evening. On Tuesday morning, at one o'clock, the administration of ether and chloroform was attempted; but the approach of the sponge or napkin occasioned such violent convulsions that the attempt was given up; spitting of thin mucus began at this time, and continued to increase till his death. Tinet. opii, in doses from 30gtt. to 3 i. was given until seven A. M. He had two intervals of sleep in the time, awaking after each with sense of suffocation. At one time his pulse became very rapid and feeble, the face was collapsed, he seemed perfectly quiet, and said he was going to die: soon after, the pulse rallied, and the spasms returned with increased violence. The perspiration, particularly about the face and neck, became very profuse, the application of a napkin produced severe convulsions. The sense of hearing seemed much exalted—he complained of the singing of a small spirit lamp, referring the sound to his own throat. The mind seemed perfectly clear, though easily irritated: he exclaimed frequently, "Oh dear! I wish I wasn't so fidgety," and to the time of his death expressing great anxiety that the fears of his mother should not be excited. At nine A. M. there was an interval of calm; the spasms were very slight, he took warm drinks from a spoon with slight effort; cold water seemed, however, to occasion great distress. Pulse 120, skin warm and moist, tongue clean. In half an hour, the symptoms became more aggravated than ever, and continued without cessation till his death. He complained much of his throat, saying there was something, rolled up like a cigar, sticking in it, which he was constantly trying to spit out; the convulsions soon became frightfully violent, until two, P. M., when, though nearly pulseless, it was decided to administer chloroform. He died at half-past two, while under the influence of this agent. The following day, at ten A. M., an autopsy was made. Decomposition had already commenced on the chest and abdomen: the body had a very offensive odour of putrefaction. The cicatrices of the wounds were somewhat bluish, with some induration of skin around them. The substance of brain and spinal chord was much softened; the distinction between gray and white cerebral matter was very strongly marked. A careful examina-

tion of other organs of the body revealed nothing calculated to throw light upon the nature of the disease.

The third case occurred at the Mass. Gen. Hospital, in the service of Dr. Cabot. The patient was a girl seven years old. She was bitten in the country, Dec. 18, 1853, and was brought to the Hospital immediately—she was seen nine hours after the accident, and found to have three wounds near the elbow, one on the palm of the hand and one on the forehead: they were thoroughly cauterized with nitrate of silver. The next day ether was administered, and the wound excised, and cauterized with nitric acid: they healed without trouble, and the patient was discharged Dec. 18th. She was re-admitted, Jan. 20th. After leaving Hospital, had appeared unusually timid, but in other respects in usual health. The first convulsion occurred the morning of her re-admission, at breakfast, when she suddenly dropped her tumbler of water and complained of inability to swallow. Since which time she has had spasms, lasting for half a minute, like catching the breath in a cold shower bath, a current of air, ray of light, sound of running liquid, or mention of them, will bring on a paroxysm. She is unable to swallow, and afraid of every one: cicatrices of wounds more red than when discharged from Hospital. A large blister was applied to the back of the neck, with rubefacient lotions to lower extremities. She slept but little the first night, but was more calm the day following, ate a piece of ice, drank some water—in the evening grew more restless, was unable to stay in bed, says she shall die to-night, spits a good deal of brownish saliva, says there is vinegar in her stomach; skin dry, tongue red, pulse frequent. Hydrocyanic acid was given, without any effect upon the spasms, which increased in rapidity and severity. She was put under the influence of ether, and its use was continued till death, Jan. 22d. At the autopsy, the brain was rather livid, the gray portion much darker than usual; nothing remarkable was found in any of the other organs.

These cases, the leading features of which have been presented, afford an ample field for professional speculation, with regard both to the nature of the malady and its treatment. The latter consideration would probably vary very much according to the heroic or expectant tendencies of each practitioner, and his confidence in the efficacy of remedies. The former, notwithstanding the great consideration which has been bestowed upon it, is still involved in obscurity. In the cases cited, two of the dogs are known to have bitten others of their species, producing, in each case, hydrophobia. In the first case—a man and boy were bitten, the first cauterized the wound freely, to the second nothing was done—neither has yet exhibited any sign of the disease. The first and last patients had their wounds thoroughly cauterized—while to the second nothing was done—yet there was no difference in the development or termination of the malady. That

hydrophobia is produced in the human race by the absorption of virus, from a rabid animal, seems beyond contradiction, from numerous observations. Still, in the progress of the disease itself there is not a recorded observation of any apparent affection of the absorbent or lymphatic system. The wound heals kindly; there is no inflammation nor tenderness of the lymphatic vessels or glands; the limb or parts adjacent to the wound is never swollen nor livid; in short, there is no one symptom of poison by absorption, such as is exhibited by the introduction of every known animal or vegetable poison, or of pus, into the vessels. On the contrary, from the moment of the bite from the animal, the disease assumes all the forms peculiar to spasmodic nervous affections. There is a strong analogy between its opening symptoms and purely hysterical affections of the throat, from intestinal or uterine irritations. In its progress and termination it resembles tetanus too much not to be ranked in the same class of diseases. The point peculiar to hydrophobia seems to be that the virus, after absorption, should affect the nervous system to the exclusion of the circulating, and that, even in such a way as to leave no trace of its progress after death.

ALPHA.

April 15, 1854.

CONSERVATIVE SURGERY.—By private advices from London we learn that Henry Hancock, Esq., Surgeon to the Charing Cross Hospital, of that city, has recently succeeded, in two instances, in saving a foot that would, but a very few years ago, have been amputated. In one case it was accomplished by resecting the astragalus—in the other the scaphoid bone. Such are among the triumphs of the art.

PORTRAIT OF FERGUSSON.—We have had the pleasure of seeing a beautiful lithograph of this distinguished surgeon. It is a copy of one not long since issued in London, and is pronounced to be a very excellent likeness. It has been issued by an ardent admirer of this eminent man, as a testimonial of his regard for him. The edition is limited; but copies can now be obtained in this city, of Bailliere, of Garrigue & Christern, or of Evans & Dickerson. The price is one dollar and a half.

NOTICE.—With the 1st of May, the office of the MONTHLY is changed. Will our correspondents and exchanges be kind enough to observe the direction on the cover?